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ABSTRACT

Papers on a variety of linguistic topics include six papers from a Festschrift and nine others, as follows: "Attitudes Towards English as a Possible Lingua Franca in Switzerland" (Urs Durmuller); "Functional Stability and Structural Levelling of Dialects: The Case of Maastricht" (Anton M. Hagen, Henk Munstermann); "On the Limits of Auditory Transcription: A Sociophonetic Approach" (Paul Kerswill, Susan Wright); "Current Language Planning and Policy in Catalonia" (Clare Mar-Molinero); "Constructive Beliefs and Political Reference" (John Wilson); Lexical Density in Interview and Conversation" (Subhi Zora, Catherine Johns-Lewis); "Reflections on Nominal Quantification in Three Romance Varieties: French, Italian, and Genoese" (Adrian Battye); "On the Notion of the Idiomatic Preposition: A Case Study from Italian" (Piero Bottari); "A Solution to the 'Must Of' Problem" (Richard Coates); "The 'No Crossing Constraint' in Autosegmental Phonology" (John Coleman, John Local); "Phrase Structure, Possessives, and Definiteness" (Christopher Lyons); "Processing Relative Clauses in Basque and Spanish" (Amaya Mendikoetxea); "A Pragmatic View of French Deixis" (John Charles Smith); "Confirmation and Repair: An Interactional Analysis of Redoing Sequences in Child-Adult Talk" (Clare Tarplee); and "The Range of Gapping and the Status of Auxiliaries" (Anthony R. Warner). (MSE)

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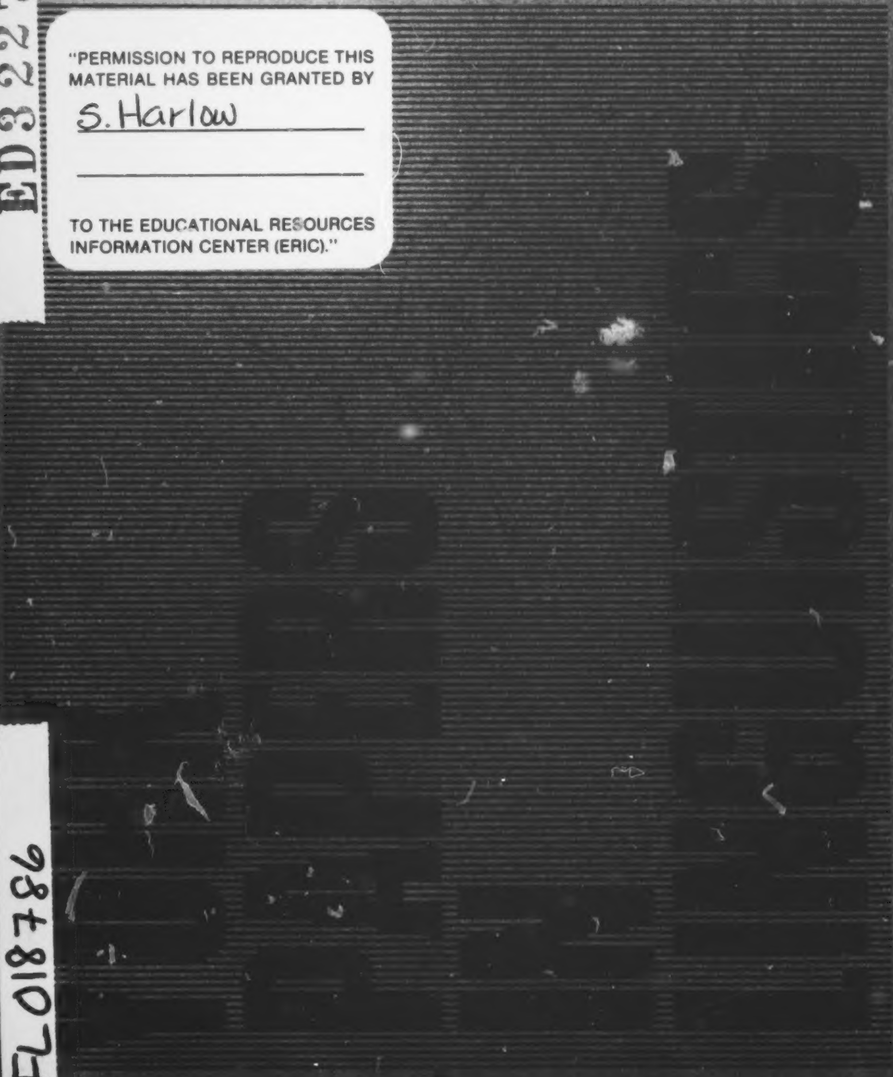

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York Papers In Linguistics

14

Editors

SJ Harlow
AR Warner

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DEPARTMENT OF LANGUAGE AND LINGUISTIC SCIENCE
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EDITORIAL STATEMENT

From this issue, York Papers in Linguistics will appear in a new and more compact format. It will have a more professional appearance which will have the pleasing by-product of enabling us to keep prices down.

The copy for the printers is produced using Microsoft Word on an Apple Macintosh and it would be of considerable assistance to the editors if contributors could submit copy on a disk in this format. Failing this, MacWrite files on a Macintosh disk, together with a hard copy of the article, or an ASCII version of the text (in *any* disk format), again with accompanying hard copy, would be acceptable.

This first part of this volume contains six papers which were submitted for inclusion in the Festschrift for Bob Le Page which formed volume 13 of York Papers in Linguistics. We are very grateful to the authors for allowing their work to appear in this, the succeeding volume. This decision was forced on us by the number of contributions to the Le Page volume and their implications for cost and size.

SJ Harlow
AR Warner

ATTITUDES TOWARDS ENGLISH AS A POSSIBLE LINGUA FRANCA IN SWITZERLAND[*]

Urs Dürmüller

University of Bern

In the following I am going to comment on statistical findings relating to the attitudes of young Swiss citizens towards English as a possible lingua franca in their multilingual country. The data are taken from a highly representative survey conducted in 1985, the Swiss Military Recruit Questionnaire filled in by 33,826 young males, aged ca. 20, from all parts and all social classes of Switzerland. For computer analysis a sample was taken: 1930 questionnaires from German-speaking Switzerland, 1133 from French-speaking Switzerland, 1256 from Italian-speaking Switzerland. In 1987 this survey was followed up by one of comparable young women. Here the figures are less impressive, but still representative. There are 324 women's questionnaires from German-speaking Switzerland, 327 from French-speaking Switzerland, and 326 from Italian-speaking Switzerland. Although data on Romantch speakers are also available, they are not considered here. They are too small a group to allow direct comparison.[1]

The term 'lingua franca' may need some clarification. The following labels for English are available: English as a native language, English as a second language, English as an additional language, English as an international language, English as a foreign language. In

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[1] **Full information on the questionnaire, time allotted for answering, surveillance and controlling, as well as on the statistical procedures are available from the 'Languages in Switzerland' Project, Deutsches Seminar der Universität Basel, Clarastrasse 13, CH-4058 BASEL. A full report is scheduled for publication in 1990.**

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Switzerland English is neither a native nor a second language, i.e. it is not the mother-tongue of the majority of the population (nor even of a sizeable minority), nor is it the language picked up by a minority speaking a different language as would be the case in an overwhelmingly English-speaking country. But English in Switzerland qualifies for all the other category labels.

English is an additional language if used within a country, often for educational purposes, also as a medium of education, especially at secondary and tertiary levels. Professional, scientific, even literary texts may be written in English. English may be used for intra-national communication between communities in the country that do not share the same mother tongue. The interaction between English as an additional language and these mother tongues may, in the course of time, produce so-called indigenized varieties of English, local forms of English. Of course, Switzerland is not India and not Nigeria, to quote two countries in which English is used as an additional language. There is no official status for English in Switzerland. But the linguistic diversity of the country surely encourages the use of English for intra-national purposes, especially if English is already used as an international and foreign language.

English as an international language is used for communication with countries abroad, for international contact in various domains. Like so many other countries in the modern world, Switzerland uses English as an international language in business, trade, politics and science contacts with other countries.

English as a foreign language occurs where English has no particular status or function within or outside the country. This is the traditional place of English in Switzerland: knowledge of English gives access to the culture, civilization and literature of the English-speaking nations.

By adopting the term 'lingua franca' I want to suggest right away that the status of English within multilingual Switzerland is moving from that of a mere foreign language to that of a language that has potential, not only for international communication, but also for intra-national communication. This development is generally perceived as unsettling the traditional linguistic stability of multilingual Switzerland. Communication between the different language groups has been ensured by enforced bilingualism. Ideally every Swiss would have at least a two-language repertoire, like this:

	L1	L2	L3	L4
German	German	French	Italian	English
Speaker	German	French	English	Italian
French	French	German	Italian	English
Speaker	French	German	English	Italian
Italian	Italian	French	German	English
Speaker	Italian	French	English	German

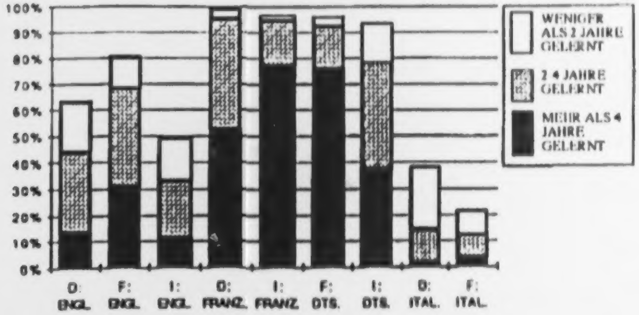
so that two speakers from different language areas would at least have one Swiss national language in common. The threat to this solution seems to come from the possibility that English might be moved to the second position in the language repertoires of Swiss speakers:

	L1	L2	L3	L4
German speakers	German	ENGLISH	French	Italian
French speakers	French	ENGLISH	German	Italian
Italian speakers	Italian	ENGLISH	French	German

so that a non-national language would become the instrument of intra-national communication, and English could be truly termed the lingua franca of multi-lingual Switzerland.

The 1985 and 1987 data show that the traditional Swiss way of ensuring intra-national communication is still being applied, and it probably still works. But people are no longer satisfied with learning a second national language only, as they have to; a lot of them also want to learn English: between 40% and 55% of the men and between 50% and 80% of the women have also learnt English. This at the cost of yet another national language. The time and energy invested in English language learning might also be seen as being missing in the study of the second national language.

The situation in Italian-speaking Switzerland seems to be different from that in German- or French-speaking Switzerland. The Italian speakers are in a minority position (4.4% of the total Swiss population) and thus feel a greater practical need to acquire not only a second, but also a third national language. English therefore comes last among what were called *Fremdsprachen* (not simply foreign languages, but also different Swiss languages) in the survey.

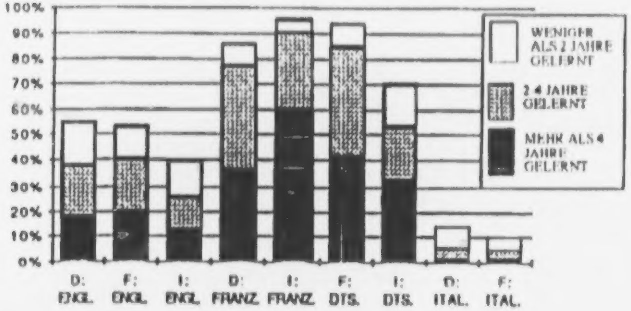


HOW LONG DID YOU STUDY OR HAVE YOU BEEN STUDYING THE FOLLOWING FOREIGN LANGUAGES AT SCHOOL? FEMALE RESPONSES:

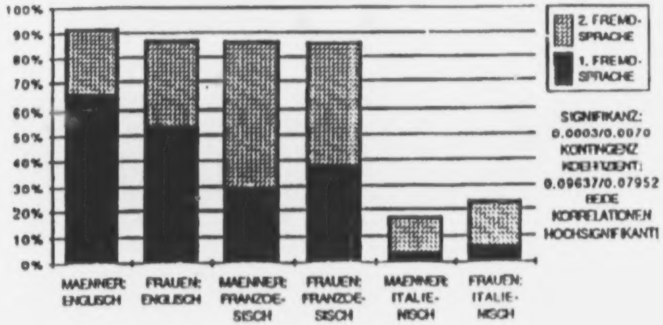
WIE LANGE HABEN SIE DIE FOLGENDEN FREMDSPRACHEN IN DER SCHULE GELERNT?

The fear of politicians and language purists that English is invading Switzerland is not without foundation. And the 20-year-old would indeed like to have the possibility of learning English right away. If English tuition were available earlier, most of the Swiss from German and French speaking Switzerland would take up English as the second language in their repertoires; German/French would drop to third position. If given this option, even the Italian Swiss would select English first; but French and German are not as far behind as the comparable Swiss national languages in German- and French-speaking Switzerland.

HOW LONG DID YOU STUDY OR HAVE YOU BEEN STUDYING THE FOLLOWING FOREIGN LANGUAGES AT SCHOOL? MALE RESPONSES:
WIE LANGE HABEN SIE DIE FOLGENDEN FREMDSPRACHEN IN DER SCHULE GELERNT?

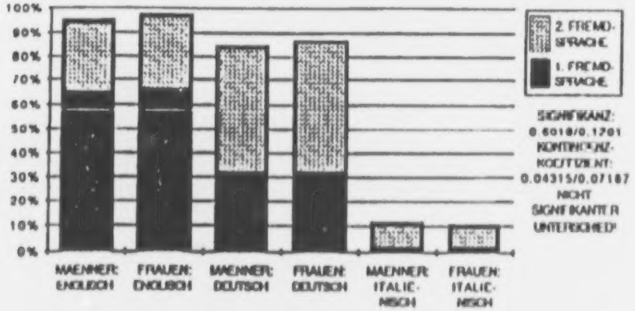


GERMAN QUESTIONNAIRES: WHICH LANGUAGE WOULD YOU PREFER TO LEARN AT SCHOOL AS A FIRST/SECOND FOREIGN LANGUAGE?
DEUTSCHE FRAGEBOGEN: ANTEILE DER PROBANDEN, DIE IN DER SCHULE ALS ERSTE//ZWEITE FREMDSPRACHE FOLGENDE SPRACHEN WÜNSCHEN:



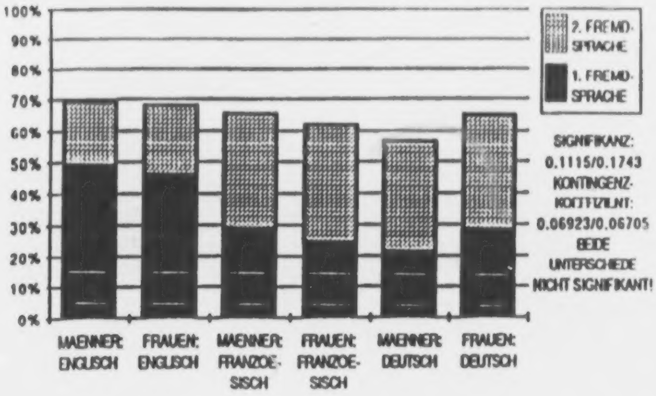
FRENCH QUESTIONNAIRES: WHICH LANGUAGE WOULD YOU PREFER TO LEARN AT SCHOOL AS A FIRST/SECOND FOREIGN LANGUAGE?

FRANZÖSISCHE FRAGEBOGEN: ANTEILE DER PROBANDEN, DIE IN DER SCHULE ALS ERSTE//ZWEITE FREMDSPRACHE FOLGENDE SPRACHEN WÜNSCHEN:



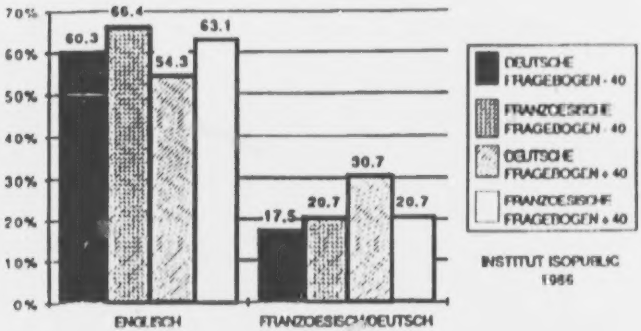
ITALIAN QUESTIONNAIRES: WHICH LANGUAGE WOULD YOU PREFER TO LEARN AT SCHOOL AS A FIRST/SECOND FOREIGN LANGUAGE?

ITALIENISCHE FRAGEBOGEN: ANTEILE DER PROBANDEN, DIE IN DER SCHULE ALS ERSTE//ZWEITE FREMDSPRACHE FOLGENDE SPRACHEN WÜNSCHEN:



In 1986, the same question was asked of the general population in German- and French-speaking Switzerland, and the same trend to place English first emerged. Even people over 40 seemed to be saying that the world has changed and that English would be their preference if they could choose today.

PLEASE IMAGINE THAT YOU ARE GOING TO SCHOOL AGAIN AND THAT YOU CAN SELECT FOREIGN LANGUAGES: WHICH LANGUAGE WOULD BE YOUR FIRST CHOICE? STELLEN SIE SICH BITTE FOR, SIE GEHEN NOCHMALS ZUR SCHULE UND KÖNNEN DIE FREMDSPRACHEN WÄHLEN. WELCHE FREMDSPRACHE WÜRDEN SIE ALS ERSTE WÄHLEN?



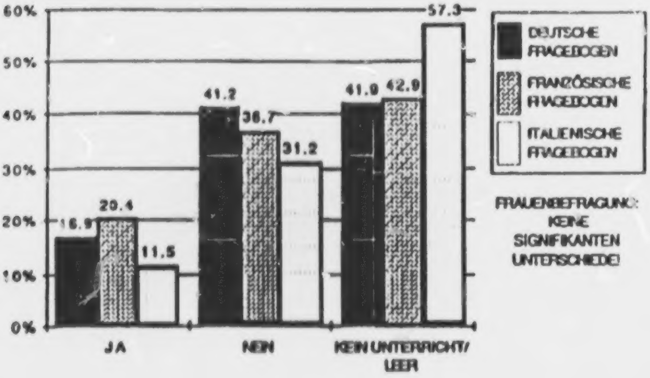
INSTITUT ISOPUBLIC 1986

The learning of foreign languages is considered important by 40 - 45% of the young men and about 90% of the young women. Note the enormous difference here between the sex groups! Indeed, most have learnt - have had to learn - a second language, and many even a third. Such positive answers to a question like 'Do you think that learning foreign/other languages is necessary?' are what one traditionally expects of Swiss citizens. However, the conventional assumption that Swiss language learners are motivated by the insight that they ought to know the languages of the other Swiss can no longer be made. Too obvious is the wish to see other Swiss languages in the curriculum replaced by English. The positive answers to the question are no doubt also motivated by their practical need to know English - a non-Swiss language.

This interpretation is strengthened by the indication of the young Swiss that they have not had enough English instruction at school.

DO YOU HAVE THE IMPRESSION THAT YOU LEARNED ENOUGH ENGLISH AT SCHOOL?

HABEN SIE GENÜGEND ENGLISCH GELERNT IN DER SCHULE?



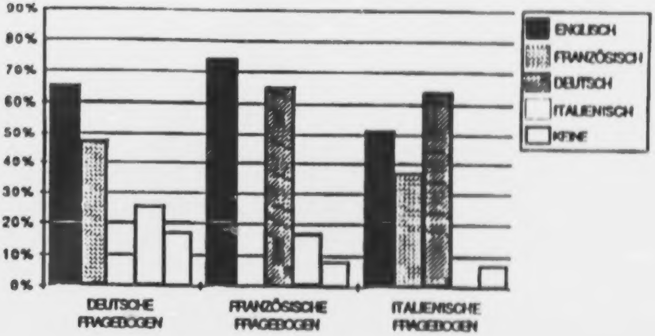
Not surprisingly, therefore, between 69% and 77% of the men and between 75% and 90% of the women think that schools should intensify foreign language instruction in English.

But why is there this clearly expressed wish for more English? With German, French and Italian, Switzerland has at its disposal very highly developed languages with long traditions and rich cultural heritages. The factors favouring the spread of English in Switzerland are the same as elsewhere in the modern world: apart from the linguistic diversity of a multilingual country, these are, particularly, material incentives and cultural affiliation (Dürmüller 1986: 11-26).

The data on the young Swiss show that already at age 20, they are expected to use in the professional sphere not only the Swiss national languages, but also English. In French and German speaking Switzerland, English is even more often required than Italian. (E: 27 - 31%, I: 12 - 20%)

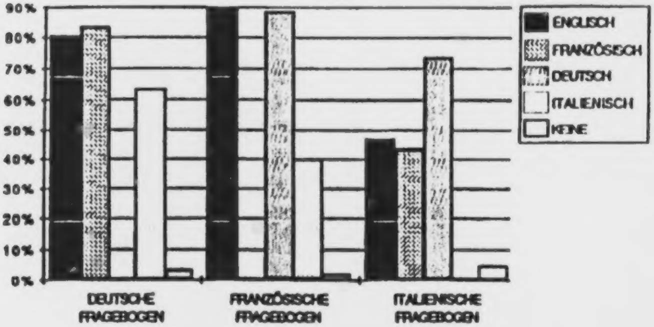
When checking on how the young Swiss see their everyday linguistic future, English came to rank very prominently as a language they expect to be using in their professional careers. English does not outshine the local Swiss languages, but it becomes obvious that it is expected to be as important as a second Swiss language. This tendency is marked especially among the women.

WHICH FOREIGN LANGUAGE DO YOU EXPECT TO USE IN YOUR PROFESSIONAL CAREER? MALE RESPONSES:
WELCHE FREMDSPRACHEN SCHEINEN IHNEN IM IHREM GEGENWÄRTIGEN ODER ZUKÜNFTIGEN BERUF BESONDERS WICHTIG ZU SEIN?



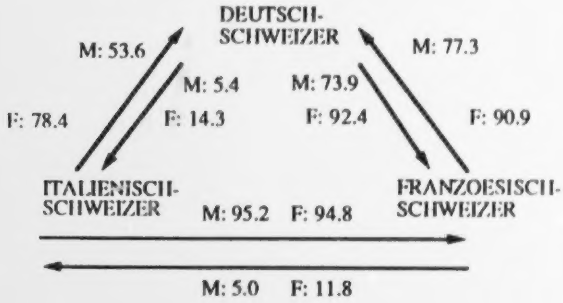
Given Switzerland's embedding within the Western world, the fact that the use of English is profession-related even within the country, cannot be surprising. English as an additional language for special purposes, might be acceptable. The question, however, is whether English might not spread further. Might it not serve the purposes of a language of wider communication, of a lingua franca? Once a substantial portion of the population speak English additionally in their workplace, can they be stopped from using it on other occasions, specifically when meeting a Swiss from a different language area?

WHICH FOREIGN LANGUAGE DO YOU EXPECT TO USE IN YOUR PROFESSIONAL CAREER? FEMALE RESPONSES:
WELCHE FREMDSPRACHEN SCHEINEN IHNEN IM IHREM GEGENWÄRTIGEN ODER ZUKÜNFTIGEN BERUF BESONDERS WICHTIG ZU SEIN?



Contact linguistic situations are quite normal for Swiss citizens - for two thirds (even 80% of the women) in our survey. But so far, no clear indications about how they communicate across the language barriers have been available. We should suppose that they are well equipped for communication in at least one other language. If we assume that a minimum period of two years of foreign/other language learning is necessary to obtain some degree of communicative competence including basic language skills, we do get the impression that Swiss bilingual education works. The arrows in the following chart show which languages that have been learnt for a minimum of two years are available to which percentage of young men (M) or women (F) when contacting inhabitants of another linguistic area. For example, 78.4% of the young Italian Swiss women have enough German knowledge to communicate with people from German-speaking Switzerland, but only 5.4% of the German-speaking males have a comparable competence in Italian.

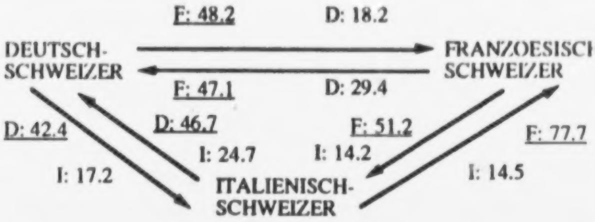
PERCENTAGE OF SPEAKERS TAUGHT OTHER SWISS NATIONAL LANGUAGES FOR A PERIOD EXCEEDING TWO YEARS:
ANTEIL PROBANDEN, DIE DIE SPRACHE DER ANDEREN SPRACHREGIONEN DER SCHWEIZ LÄNGER ALS ZWEI JAHREN IN DER SCHULE GELERNT HABEN:



The chart illustrates that communication in one language channel at least should always work. It also shows that the minority group of the Italian Swiss carry the heaviest burden in this intra-national communication scheme: to a large extent they have acquired not only one other Swiss language, but both French and German!

English doesn't do badly either: For a third of the young Swiss it might serve the purposes of a lingua franca at least theoretically. The percentages are between 37.4 (for German-speaking males) and 67.7 (for French-speaking women). When asked whether they actually did use the other languages when together with people from different language areas, the figures began to look somewhat different. They show now that there is a bilingual imbalance between the linguistic groups of Switzerland: if Italian-speaking Swiss meet with either German- or French-speaking Swiss, the language of communication is predictably not Italian, it is either German or French. If French-speaking Swiss meet with either Italian- or German-speaking Swiss, the language of communication is predictably French, not Italian or German. The German-speaking Swiss hold a middle ground: they yield to the French speakers, but not to the Italian speakers.

WHICH LANGUAGE DO YOU USE IN COMMUNICATION WITH SWISS PEOPLE WHO SPEAK A LANGUAGE OTHER THAN YOUR MOTHER TONGUE?
IN WELCHER SPRACHE UNTERHALTEN SIE MIT ANDERSSPRACHIGEN SCHWEIZERN? IN DEN FOLGENDEN KOMMUNIKATIONSSITUATIONEN UNTER VERSCHIEDENSPRACHIGEN SCHWEIZERN SIND DIE DOMINIERENDEN SPRACHEN:

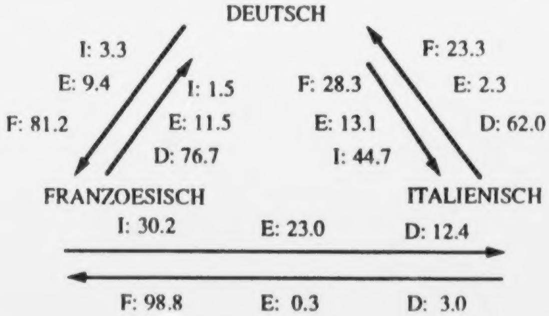


In this situation of bilingual imbalance, where one language group seems quite unwilling to use another language (the French) and another language group mostly has to use other languages (the Italian), the chances for English to be used as a language for intra-national communication may be seen as increasing. As a foreign language, English is not tied up with any particular community of Switzerland. It is a neutral tool, available to all the linguistic groups in the same way, not putting any one group at an advantage or at a disadvantage.

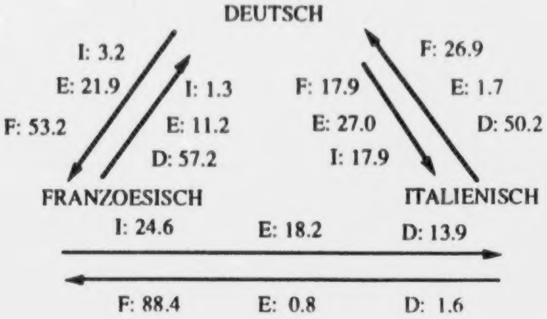
Already today, English is sometimes chosen as the lingua franca between speakers of different linguistic background if their mother tongue cannot be used. Of course, the second Swiss language, which was taught at school to practically all the Swiss (see above), comes first; but English is the second choice in this situation, before the third national language. With the trilingual Italian speakers, English is third choice, but with German-speaking males conversing with Italian speakers it is number one, even before French.

In the light of these and many more data which it was impossible to present here, the possibility that English might increasingly be used as a lingua franca in multilingual Switzerland must be taken seriously. Not many people would like to adopt English officially as an additional language in Switzerland. Also, the 20-year-olds are clearly against an introduction of English. However, they consider it quite conceivable that one day English might be made a Swiss national language.

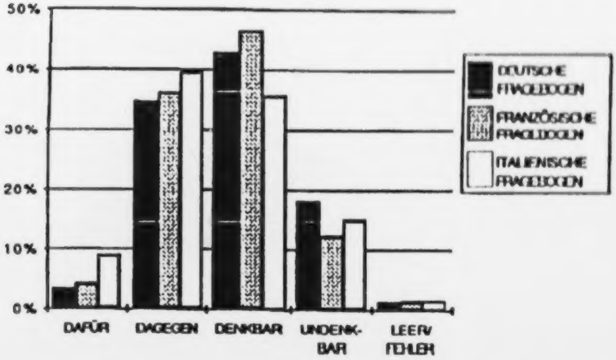
WHICH LANGUAGE DO YOU SPEAK WITH ANOTHER SWISS WHO DOES NOT SPEAK YOUR MOTHER TONGUE? FEMALE RESPONSES:
IN WELCHER SPRACHE UNTERHALTEN SIE MIT EINEM ANDERSSPRACHIGEN SCHWEIZER, DER IHREN MUTTERSPRACHE NICHT SPRICHT? FRAUEN:


DEUTSCH
I: 3.3
E: 9.4
F: 81.2
I: 1.5
E: 11.5
D: 76.7
F: 28.3
E: 13.1
I: 44.7
F: 23.3
E: 2.3
D: 62.0
FRANZOESISCH
ITALIENISCH
I: 30.2
E: 23.0
D: 12.4
F: 98.8
E: 0.3
D: 3.0

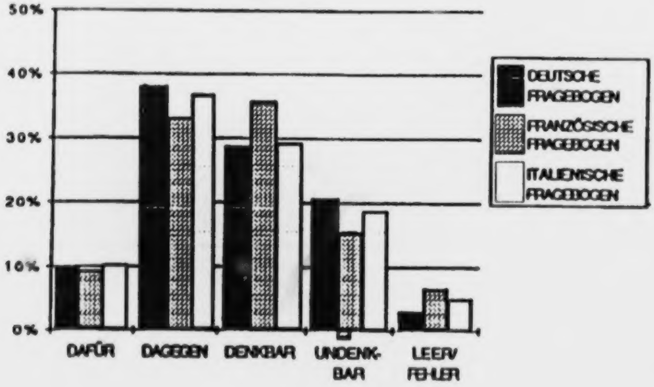
WHICH LANGUAGE DO YOU SPEAK WITH ANOTHER SWISS WHO DOES NOT SPEAK YOUR MOTHER TONGUE? MALE RESPONSES:
IN WELCHER SPRACHE UNTERHALTEN SIE MIT EINEM ANDERSSPRACHIGEN SCHWEIZER, DER IHREN MUTTERSPRACHE NICHT SPRICHT? MÄNNER:


DEUTSCH
I: 3.2
E: 21.9
F: 53.2
I: 1.3
E: 11.2
D: 57.2
F: 17.9
E: 27.0
I: 17.9
F: 26.9
E: 1.7
D: 50.2
FRANZOESISCH
ITALIENISCH
I: 24.6
E: 18.2
D: 13.9
F: 88.4
E: 0.8
D: 1.6

WHAT WOULD YOU THINK OF MAKING ENGLISH A SWISS NATIONAL LANGUAGE?
FEMALE RESPONSES:
WAS MEINEN SIE ZUM VORSCHLAG, AUCH DAS ENGLISCHE ZUR OFFIZIELLEN LANDESSPRACHE ZU ERKLÄREN?



WHAT WOULD YOU THINK OF MAKING ENGLISH A SWISS NATIONAL LANGUAGE?
MALE RESPONSES:
WAS MEINEN SIE ZUM VORSCHLAG, AUCH DAS ENGLISCHE ZUR OFFIZIELLEN LANDESSPRACHE ZU ERKLÄREN?



Today Switzerland is left in a somewhat schizophrenic situation: the general population and the politicians want to preserve the traditional status of multilingual Switzerland. The idea of accepting English officially is clearly resented. Yet, a lot of English is allowed into the country through a multitude of channels. English-language movies are shown with their original sound track, and this is welcomed by the people. English-language music is on the air of Swiss radio stations all the time, and again no one would ask for translations. Swiss people claim to read as many books in English as in a second Swiss language; they write more graffiti in English than in a second Swiss language; and they do not hesitate to apply for jobs that require fluency in English.

English is accepted as an additional language, a language for specific purposes, and it is accepted as a language for international communication. However, it is not yet accepted as a language for intranational communication, although its use in job-related and leisure-time domains makes it increasingly more difficult not to resort to English as a possible lingua franca in a multilingual setting.

REFERENCE

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FUNCTIONAL STABILITY AND STRUCTURAL LEVELLING OF DIALECTS: THE CASE OF MAASTRICHT[*]

Anton M Hagen and Henk Münstermann

University of Nijmegen

1. Introduction

In studies of loss and maintenance of minority languages and dialects, it is usually argued that there is a strong relation between the loss of functions and a decreasing use of the language and the one hand, and the loss of structural elements of that language on the other. That functional reduction goes hand in hand with structural reduction has been demonstrated for instance in studies by Haas (1968) on Biloxi, Dorian (1981) on Gaelic, Dressler and Wodak-Leodolter (1977) on Breton and in a lot of other studies on minority languages. Also in studies on dialect levelling, e.g. of Kristensen and Thelander (1984) in Denmark and Sweden, and of Peter Trudgill (1986) in his *Dialects in Contact*, the central presupposition is that dialects change and undergo structural reduction under both functional and linguistic pressure from the standard language.

In the Department of Dialectology at Nijmegen University, the relation between functional and structural dialect loss is now being investigated in a whole range of communities in the Dutch province of Limburg (between Ottersum in the north and Maastricht in the south. Cf. figure 1). Progress reports on this research can be found in Hagen (1986).

The research in Limburg suggests that the relation between functional and structural loss can be rather less straightforward than is gen-

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erally taken for granted. Especially relevant in this respect is the ongoing research into the city dialect of Maastricht (Münstermann and Hagen 1986, Münstermann 1988). We shall present some data drawn from this research. They show only minor changes in the very favourable attitudes towards the dialect, and quite stable patterns of domain use, but, on the other hand, also reveal a substantial loss and reduction on the level of the language structure.

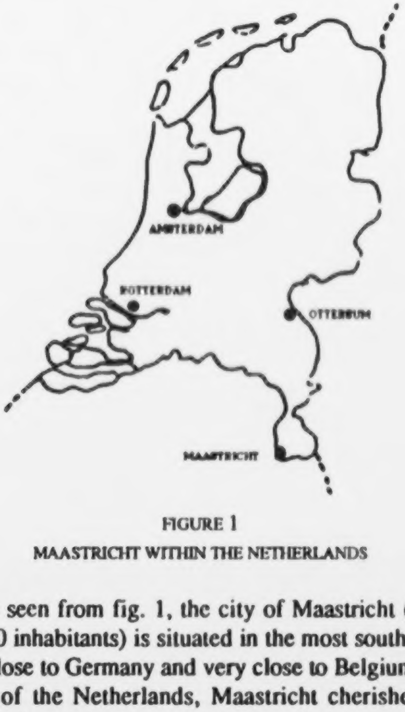

FIGURE 1
MAASTRICHT WITHIN THE NETHERLANDS

As can be seen from fig. 1, the city of Maastricht (with approximately 110,000 inhabitants) is situated in the most southern part of the Netherlands, close to Germany and very close to Belgium. As the most southern part of the Netherlands, Maastricht cherishes a southern, romanic-like and, some say, even a Burgundian image. There are, of course, historical reasons for this rather unique position of Maastricht among the cities of the Netherlands. One of them is that the city, both because of its peripheral Dutch situation, and because of its strategic and commercial importance, had a rather autonomous cultural development until far into the nineteenth century. Until that time the linguistic situation in the city could roughly be described as di- or triglossic with French and standard Dutch as competing high varieties and the dialect of Maastricht as the common variety.

Nowadays, the dialect spoken in this city is still characterised by considerable linguistic distance from the Dutch language on the levels of phonology, morphology and lexicon. Furthermore, the dialect still has a considerable number of speakers. The use of the dialect is, unlike most city dialects in the Netherlands (cf. Hagen and Giesbers 1988), not limited to one particular social class. In fact, the distribution of the dialect and standard language seems to be functional rather than social.

2. Method

The subjects in this study were 64 inhabitants of Maastricht, all born and raised in this city. The sample was stratified for age, for obvious reasons. There were three generations: an older generation (over 55 years), a middle generation (30-45 years), and a younger generation (15-20 years). Furthermore, the sample was stratified for neighbourhood and sex; but in this presentation we will not go into these last variables.

In the Maastricht study dialect loss was treated as a linguistic phenomenon, as well as a sociological phenomenon. Dialect loss in a linguistic sense was measured by a number of production and acceptability tests, most of them on morphological procedures. These tests can be described as 'direct discrete point tests', (cf. Oxford, 1982). The elicitation procedures used can be compared with those in Greenbaum & Quirk (1970). This implies that they are directed towards the elicitation of variants of concrete variables. As a consequence, there was an almost maximal monitoring by the informants. Therefore, one can say that the tests measure knowledge of the dialect (competence), rather than the actual use of the dialect (performance). In case an original variant of a variable could not be elicited, this variant was considered unknown and therefore lost.

Dialect loss in the sense of the sociology of language, that is in terms of language shift, means the loss of functions the dialect is used for or considered suitable for. In order to assess the relative prestige of the local dialect as well as its function as a symbol of local or regional identity, the functional analysis started with attitude research. Attitudes toward the Maastricht dialect, the standard language and the dialect community were measured with two attitude measurement techniques. The first was the matched-guise procedure. The informants were asked to evaluate different speakers on a list of personality traits. In fact they heard two speakers and three varieties; of course one speaker spoke Standard Dutch and Maastricht dialect. The second attitude measure was constituted by a Likert-type scaling instrument that contained 15 statements concerning loyalty towards the local dialect and the dialect community. Apart from attitudes, dialect maintenance and dialect shift on the functional level were investigated in two domain questionnaires: one for reported use, and one for subjective suitability of the use of the dialect in a number of situations.

3. Results

3.1 Functional loss

In fig. 2 the results of the matched-guise test are summarized. In this figure the classical pattern of the standard language as the more prestigious variety, and the dialect as the more attractive one, is not confirmed. For the matched speaker the status-items show about the same scores for dialect as for Standard Dutch. The differences between the two are not significant (t-tests). As for the items reflecting social attractiveness, the dialect is clearly evaluated more positively than Standard Dutch. Of the 16 items on the scale, 8 items show significant differences in evaluation between the Standard Dutch version and the dialect version (t-test 5%-level); 7 of these differences are in favour of the dialect. Without exception these items reflect social attractiveness or integrity, they all have to do with a solidarity dimension. The one item that showed a higher mean score for Standard Dutch is 'leadership', clearly a status item. The other status items show higher scores for Standard Dutch as well, but the differences found are not significant.

For the second attitude measure, directed towards statements concerning loyalty towards the dialect and the dialect community, factor analysis was used, in order to check if the dimensions thought of in

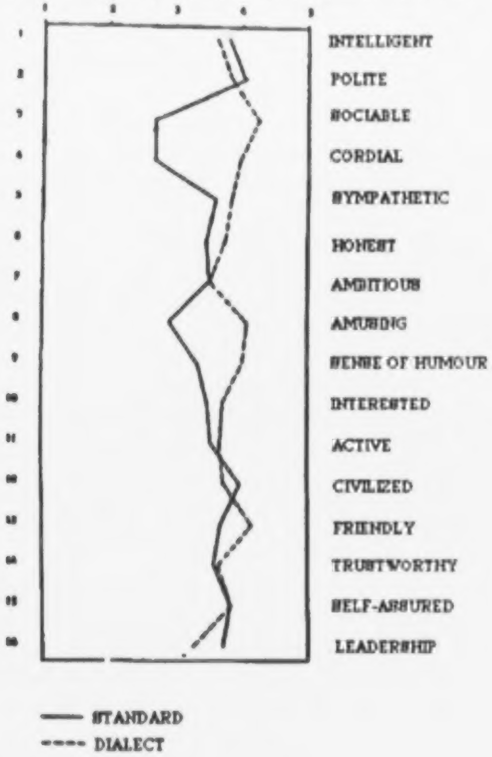

FIGURE 2

SCALING PATTERN ON THE MATCHED-GUISE TEST (FIVE-POINT SCALE) advance were indeed confirmed by the outcome of factor analysis. Three factors were extracted. The first factor contained statements on the aesthetic value of the dialect. The second factor was constituted by statements on dialect ideology. The third factor reflected loyalty towards the city and the dialect community. The summed scores for the factors were divided by the number of items in the summation, so that the scores are between 1 and 5, like the original five-point-scale scores. The mean scores are: 4.37 for the aesthetic judgment, 3.65 for ideology, and 4.20 for loyalty. These mean scores are fairly high; it is clear that the subjects have positive attitudes towards the dialect and the community. This finding confirms the outcomes of the matched-guise test.

The situations used in the two domain questionnaires could be grouped on three dimensions after factor analysis. The dimensions were labeled 'instrumentalism', 'family' and 'solidarity'. 'Instrumentalism' covers a group of situations of a transactional or formal nature. 'Solidarity' is a label for typical in-group situations. One might expect 'family' to be included in the solidarity factor, but, probably because of extremely high scores with consequently lower variance, the family situations constituted a separate factor.

As can be seen from fig. 3A on reported use, there is a considerable amount of use of dialect in the groups of domains labelled 'family' and 'solidarity' (the maximum use is 5!). There is only a slight decrease in use per generation. On the 'instrumentalism' dimension however the generation effect is significant. Since the 'instrumentalism' factor contains intergroup and transactional situations also with non-native or removed inhabitants or with visitors of Maastricht, and since these situations have become more frequent, the decrease of dialect in these domains is not surprising. In spite of a certain decline, the general position of the dialect in a dominance configuration is still quite strong.

As can be seen, fig. 3B shows that the scores for the suitability of dialect use are also quite high for the factors 'solidarity' and 'family'. The results however are somewhat more complicated than the reported use scores, especially on the 'family' factor. Here, a clear curvilinear pattern emerges, which means that the middle generation considers dialect less suitable for use in family situations than the other two generations. Since the middle generation is the generation with children who are still growing up, the question of the choice of variety is more salient there than in the other generations. The greater involvement of the middle generation in exactly this question most probably requires a more considered judgment.

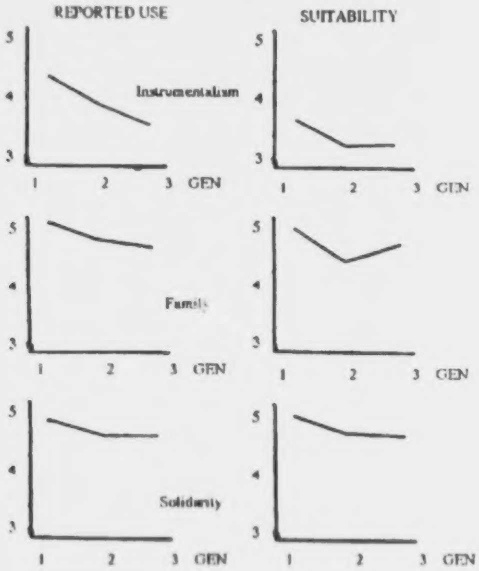

FIGURE 3
GRAPHIC REPRESENTATION OF SCORES ON THE DOMAIN QUESTIONNAIRES FOR REPORTED USE (3A) AND SUITABILITY(3B) IN THREE GENERATIONS

We will not got into a comparison between the reported use scores and the suitability scores, but restrict ourselves to the conclusion that both types of scores indicate that, in general, the position of the dialect in Maastricht is still very strong.

3.2 Structural loss

As has been indicated above, the loss of typical dialect structures was also investigated in a series of elicitation and acceptability tests. Here we will only briefly present results from some of the elicitation tests on dialect morphology. Each of the investigated morphological variables was represented by an average of 10 test items. Results are presented in table 1. The scores were calculated so that 1 is the theoretical maximum score and 0 the minimum. The table shows that even the oldest generation as a group never reaches the theoretical maximum of the NORMS-speaker; therefore the best empirical point of reference for the loss process is the oldest age group in the sample (for the problem of the 'point of reference' in language loss research, see Jaspaert, Kroon & Van Hout 1986).

	1	2	3	4	5	6	7	8
Old	0.72	0.75	0.98	0.77	0.11	0.48	0.80	0.33
Middle	0.42	0.47	0.79	0.77	0.12	0.61	0.61	0.30
Young	0.47	0.39	0.64	0.54	0.05	0.37	0.47	0.00

Table 1. Competence scores for eight morphological dialect variables in three generations

The first test has to do with the grammatical gender of nouns. The Maastricht Dialect (MD) grammar has an explicit three-gender system. Standard Dutch (SD) officially has a three gender system as well, but actually the maximum differentiation in the variable under investigation is only twofold. The MD-nouns in the test were chosen for having a gender differing from that of their SD-equivalents.

1. In the first part a gender-specific *demonstrative* pronoun had to be filled in in the MD-sentence.

MD	*Dat* febrik is aajt (neuter demonstrative) 'That factory is old'	SD	*Die* fabriek is oud (masculine demonstrative)

2. In the second context an *adjective* to the same nouns had to be filled in; the inflexion of the adjective also shows grammatical gender of the nouns.

MD	't Is 'n *aw* febrik	SD	't is 'n *oude* fabriek
	(neuter adjective)		(masculine adjective)
	'It is an old factory'		

3. The third part of the gender-test concerns *pronominal reference* of the type:

MD	Hej is en nuij taofel.	SD	Hier is een nieuwe tafel.
	Wie vinste *ze*?		Hoe vind je *hem*?
	(feminine personal pronoun)		(masculine personal pronoun)
	'Here is a new table.		
	How do you find it?'		

Results on the gender variables (table 1, column 1-3) show that for the first two contexts there is a significant decrease of the use of the genuine dialect gender in the middle and younger generations. It is rather surprising that pronominal reference seems to be less vulnerable.

4. The next variable is the vowel mutation in 2nd and 3rd person singular in the present tense of strong verbs, a phenomenon unknown to Standard Dutch; it is comparable to the A-Umlaut rule or the E-I-Wechsel in German. In 10 sentences forms with vowel mutation in the dialect had to be filled in.

vallen	:	MD	diech *vels*	SD	jij *valt*
(infinitive)	:		(2 person sg.)		
to fall			*you fall*		

Here too the attrition is evident (table 1, column 4). Yet, it starts only after the middle generation, so it is of recent date.

Variables 5 to 8 have to do with principles of morphological derivation for nouns and adjectives.

5. The first of these concerns the principle of the derivation of nouns from verb-stems by prefix *ge-* and suffix *-s*, which has a function comparable to that of substantised *-ing* forms in English. The Standard Dutch equivalent is prefix *ge* + verb-stem, so the morphological difference with the original dialect form is only the absence of suffix *-s*.

MD	*gesjriews* 'shouting'	SD	*geschreeuw*

Apparently, the contrast between dialect and Standard Dutch has almost completely disappeared (cf. table 1, column 5). This is quite imaginable if we assume that similarity in rules between two systems will provoke levelling. On the other hand, highly transparent rules are supposed to be rather resistant. A regular derivational principle like *ge*+stem+*s* certainly looks transparent. These two tendencies seem to be in conflict with one another here, and evidently similarity is the strongest. Yet we could also assume that similarity was helped in this case by the fact that the Standard Dutch derivation is simpler, because only one prefix is used, and therefore is more attractive.

6. The second derivational principle derives adjectives from verb stems with the suffix *-etig*. One SD equivalent is *erig*, but in the dialect the forms may also have the same function as the SD present participle on *-end*.

MD	*gleujetig* 'glowing'	SD	*gloeiend*

Table 1, column 6 shows that the scores spread around the middle, in other words, the use of the dialect suffix has become optional in the whole language community.

7. The third principle is derivation of nouns from adjectives with the suffix *-igheid*, corresponding to Standard Dutch *-heid* and something like English *-ness*.

MD	*meujigheid* 'tiredness'	SD	*moeheid*

This principle shows significant loss over the three generations. Nevertheless, the use in the younger generation still demonstrates optionality of the use of the dialect suffix. So, in general this principle proves to be more resistant than the other two. Probably this is due to the fact that the geographical spread of the principle is much wider than that of the other derivational variables discussed here. Not only do most dialects in the province of Limburg use this derivation, but also most Brabantine dialects. The hypotheses that more widely spread features are more resistant in processes of dialect loss is obvious from Thelander's research on dialect levelling in Sweden (cf. Thelander 1982). This finding seems to be confirmed by our data.

8. The last of the four derivational principles goes back to the Middle Dutch period. It is the derivation of nouns indicating female persons, from nouns on suffix *-er* or *-aar* indicating male persons, by adding the suffix *-se*.

MD	*naober - naoberse* (masc.) (femin.) 'neighbour - neighbour's wife'	SD	*buurman - buurvrouw* (masc.) (femin.)

Clearly this derivational principle is completely lost, as can be seen from the scores of the younger generation in table 1, column 8. Also the knowledge of the principle in the middle and older generation was not very convincing, so that we can assume that the principle has been optional for a long time.

These and other results of the structural dialect loss tests show that dialect loss is a real phenomenon in Maastricht. Not only do the results for most of the presented linguistic variables differ substantially from the theoretical maximum, but clear differences between generations have also been found. The same holds for most of the other variables in the study, that are not discussed in this paper.

4. Conclusion and discussion

As can be seen from the results presented in this paper, the functional loss of the dialect does not keep pace with the structural loss. Correlations between the linguistic variables and for instance the domain variables are extremely low. Only the gender system variables and the domain variables show correlations between .30 and .40, which is still not much if we consider the variance just discussed. Not more than 9 - 16% of the variance in the gender system variables can be explained by the degree of dialect use or subjective suitability of dialect use.

How can we explain this apparent lack of relation between functional and structural loss? First we can assume that the relation between the two processes is not a linear one, as is presupposed in the Pearson correlation coefficient. A linear relation would mean that both processes take place not only simultaneously, but also at the same pace. Evidently, the structural loss in Maastricht is at this moment way ahead on the functional loss, which does not mean of course that both processes are completely independent. A possible explanation is that the two processes of dialect loss are caused by different forces that are not necessarily related to one another. Structural loss may well be caused simply by the increasing exposure of the dialect to the standard language (school, media, increasing mobility etc.). Functional loss will increase if the dialect is not considered suitable for use any more in a particular set of situations. This of course is also a matter of the prestige of the dialect in the speech community, which in this case used to be fairly high.

It is quite imaginable that, once structural loss has gone so far that the dialect is only a regional style of the standard language, the dialect will become socially marked and will lose most of its functions, beginning with the instrumental dimension.

But instead of speculating about the future, let us try to describe the present shape of the dialect-standard situation in Maastricht. For this purpose we refer to fig. 4 which gives an abstract model of the speech repertoire in a dialect-standard situation, after Thelander's research in Burtrask, Sweden (Thelander 1982; Kristensen & Thelander 1984).

D	D	D	D	D	D	D	D	D	D	Dialect
D	D	D	D	D	S	S	S	S	S	Interlanguage
S	S	S	S	S	S	S	S	S	S	Standard

Figure 4 Dialect-Standard repertoire after Thelander (1982)

It is easy to see that, by using a 50%-criterion for a distinction in dialect- or D-indicators vs. standard- or S-indicators, also from the scores in table 1, the picture emerges of an 'interlanguage' competence, or, in Trudgill's (1986) terminology, an 'interdialect' competence of the Maastricht informants: the number of D-indicators range from 5 out of 8 in the oldest generation, to 2 out of 8 in the youngest generation. Nevertheless, the youngest generation also perceive this interlanguage as dialect use.

Probably the term *ambiglossia* characterizes rather well the language situation in which the speech community clings to the use of two varieties, but at the same time neutralizes and levels structural differences between these varieties (cf. Hagen 1983). Ambiglossia manifests itself on different levels with different phenomena: on the level of the verbal repertoire by frequent code switching and code mixing; on the level of the language variety by interdialect, which, as we have seen, is characterized by frequent substitution of dialect forms by standard language forms; finally, on the level of separate linguistic items, by intermediate forms which result from processes of merging, reduction and simplification. We would like to suggest that 'ambiglossia', much more than 'diglossia' is quite typical for the present situation in traditional Western European dialect areas.

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ON THE LIMITS OF AUDITORY TRANSCRIPTION: A SOCIOPHONETIC APPROACH*

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1. Introductory remarks

In a way, this paper only obliquely addresses sociolinguistic issues. But we can justify its inclusion in this volume in that it fits into a way of thinking that has been characteristic of a number of sociolinguists in the last eight or ten years. During this period, sociolinguistics has become something of a self-scrutinising subject, in that people have questioned not only the methodology but also the linguistic and social theory behind it. This paper can be seen as a contribution to this discussion.

However, it intends to do so in a novel way. We will tackle an area of sociolinguistic methodology which is rarely discussed; and we are going to try to show that this is of no less theoretical significance: this is the phonetic nature, and linked with that the transcription, of the actual sounds uttered by speakers. At first sight, this seems to be a purely atheoretical problem, a matter of nuts and bolts. After all, transcription is something that, with a bit of ear-training, we can all get reasonably good at; as such, it's simply a tool of the trade. But in our view that is

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not all that transcription is about; it is also part of the theory of sociolinguistics.

There are two reasons why we think this is so. Firstly, from the speakers' point of view, the sounds are what they use to convey complex indexical information. Secondly, from the point of view of the linguists, for them to do their transcription they need a phonological theory, however rudimentary. Without a theory, they cannot know what kind of detail to transcribe, and with the wrong theory they will transcribe the wrong detail. Towards the end of this paper, we will show what trained phoneticians do when they are presented with a transcription task to carry out 'cold', without any knowledge of the dialect they are listening to, and without any explicit phonological theory as a point of departure.

In fact, quite a lot of attention has been paid to the LINGUISTIC representation of the variants of phonological variables, notably by Knowles (1978), Lodge (1986), the Milroys (e.g. J. Milroy, 1976) and Harris (e.g. 1986). Regrettably (for reasons that will become clear), this has not gone hand-in-hand with a consideration of what happens during the act of transcribing those variants. This will be the central concern of this paper.

2. The importance of phonetic transcription

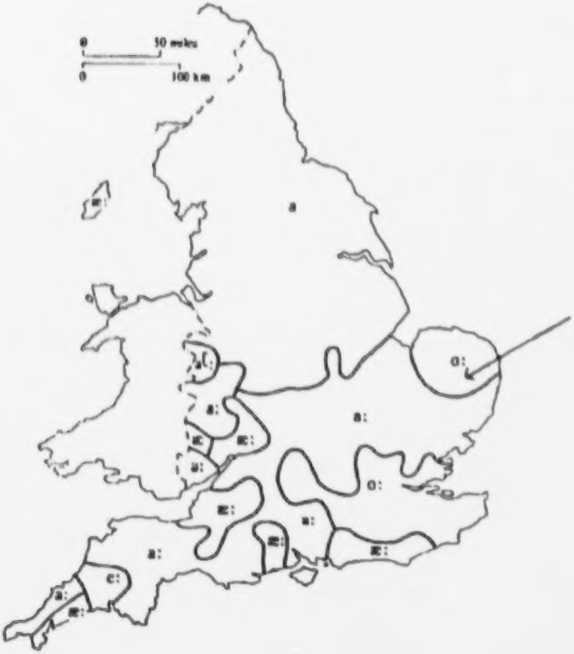
Before we look at the experiments we carried out, we will consider in more detail WHY it is important to examine phonetic transcription. We will approach this question from two angles: first, from the point of view of recent dialectology and sociolinguistics in general; and second, from the specific point of view of a more phonetics-based field which can be termed 'sociophonetics' - in particular, our own work on connected speech processes in local Cambridge English.

2.1 Dialectology and sociolinguistics

First, then, some general points about dialectology and sociolinguistics.

We can start with a rather alarming discovery made a few years ago by Peter Trudgill (1983: 31-51). On the basis of the *Linguistic Atlas of England* (Orton et al., 1978), he drew the map shown in Fig. 1.

FIGURE 1
Vowel in 'last' (from Trudgill, 1983)



This shows the reflexes of Middle English *a* in the word *last*, The point to note is this. There is a large band across the middle of England where the vowel is [a:] which separates two areas with [ɑ:], one to the south-east, the other to the north-east, in Norfolk. Trudgill was suspicious of this transcription, believing a front vowel to be more usual in Norfolk.

He noted that this Norfolk [ɑː] area (marked with an arrow) in fact covers the locations surveyed by one particular field-worker, who quite simply 'got it wrong' (1983: 40); the result is a 'field-worker isogloss' (op. cit.: 38). The moral here is obvious.

Sociolinguists and dialectolologists have relied heavily on auditory phonetic transcription as a basic analytical tool in their investigation of variation and sound change. And, as we mentioned, it has been treated by them as a pre-theoretical notion, and they have regarded it as a tedious but necessary evil. In most of the early studies, little attention was paid to the transcription itself, though the precise effect of this failure (if this is the appropriate word for it) is hard to assess. There are two important issues here. These are, first, the *reliability* and, second, the *validity* of the transcriptions. First, let us look at reliability: how consistent are transcriptions both across transcribers and within transcribers? The more significant of these, we think, is within-transcriber variability, since most of the transcription is usually done by a single person. The main question here is whether or not a transcriber is consistent: will he or she transcribe the same token the same way twice? And does that transcriber have a tendency to 'drift' in his or her judgments over a period of time? We shall not in this paper have any more to say on the subject of reliability. We shall be more concerned with the validity of the transcriptions. Here, the main question is the way in which a transcription reflects (a) articulatory facts and (b) auditory impressions. So we might like to consider whether there is a consistent bias towards a particular transcription in, say, a particular phonological environment, or whether manner of articulation influences the perception of place of articulation.

We mentioned earlier the increasing sophistication of the linguistic variable. Just to give some idea of how complex a variable can become, consider Table 1, which shows one linguist's analysis of the variants of two vowels in Liverpool:

TABLE 1
/ʊə/ and /ɔə/ in Liverpool (from Knowles, 1978: 85)

		sure /ʊə/	*shore* /ɔə/
1	Lax [ʊ,ɔ] before an unstressed vowel:	ʊə	ɔə
	or		
2	(a) diphthongize [u,o]:	ɪúə́, ɪ́uə́	ɔ́uə
	or		
	(b) front [u]:	ʉə	
3	modify VVV to V + glide + V:	ɪwə	ɔwə
4	front final [ə]:	uɛ, ?ʊɛ	oɛ, ?ɔɛ
		ɪúɛ, ɪ́uɛ	ɔ́uɛ
		ʉɛ, ɪwɛ	ɔwɛ

He sees these variants as generated by a set of interacting rules, which represent 'the options open to the speaker at different stages in speech production, and the way these options can be used to convey sociolinguistic information about the speaker' (1978: 90). Similarly, for the consonantal variable (ng), corresponding to the velar nasal in RP *sing*, Knowles identifies the following variants, again generated by rules (op. cit.: 86):

sɪŋg	sɪŋ
sɪŋːg	sɪːŋ
sɪŋː	sɪːŋ^{g}

Knowles' analysis is multidimensional. This is true also of the Milroys' analysis of Belfast vowels. Table 2 (taken from Milroy, 1987: 124) shows the variables (o) and (ɛ), as realised in the data for a single speaker. Milroy argues that these variants should be analysed in terms of three sub-scales: roundness, backness and length.

Clearly, if sociolinguists are going to operate with this amount of detail, we need to know something about the reliability and validity of the transcriptions on which their (usually sophisticated) analyses are based.

TABLE 2
(o) and (ɛ) in Belfast (from Milroy, 1987: 124)

(o):	ɑ̝	ɑ	ɑː	ɔ	ɔː
	got		shop		
	Polytech				
	shop			probably	job
	pot	concentrated			of
		vodka			God
				bottom	

(ɛ):	ɛ	ɛː	ɛ̝	ɛ̝ː
	set-up		specials	red
	lent		went	tell
	went			ten
	specials			
	remember			
	twenty			

2.2 Sociophonetics

If Liverpool causes difficulties for the transcriber, this is even more true of a relatively new field of study, which intersects, to a greater or lesser extent, with correlational sociolinguistics. This is the growing field of sociophonetic research. A recent, though largely descriptive example is Lodge's (1986) outline of the phonetics and phonologies of a number of non-standard varieties of English. In it, he pays special attention to the word in connected speech. As in other sociolinguistic studies, Lodge uses an auditory transcription, noting quite fine detail.

Lodge is not only interested in 'traditional' phonetic variables, but also in the range of assimilations, deletions and epentheses of normal connected speech. In our work in Cambridge, we too have focused on these phenomena, which we call *connected speech processes*, or *CSPs*.

We will digress a little at this point to say something about the background to our Cambridge project, so as to make it plain just why we have conducted the transcription experiments we are going to be reporting. Unlike the 'traditional' variables of sociolinguistics, CSPs are in some sense phonetically motivated: that is, their application can be explained with reference to the physiology and the dynamics of the vocal tract. Our own interest in these phenomena derives from two sources. The first is the observation that conditioned sound changes are always the result of the fossilisation of CSPs. The second concerns the fact that CSPs tend, despite their 'naturalness', to be to some extent variety-specific. This is shown in Dressler's work in Vienna (Dressler & Wodak, 1982) and in the work of one us in Durham (Kerswill, 1987). Dressler talks about lenition and fortition rules (i.e. CSPs) which serve to ease production (in the case of lenitions) or to ease perception (in the case of fortitions). Some of these processes are apparently specific to one or other of the two major varieties of German spoken in Vienna: the local dialect and standard Austrian German. In Durham, Kerswill observed that certain processes usually described for English appeared to be absent, while others not generally found in the literature were present. The two clearest examples are those shown in Table 3, overleaf. By combining the facts of sound change and the variety-specific nature of CSPs with the sociolinguistic axiom that sounds undergoing change are sociolinguistically salient, we arrived at the basic hypothesis of our study. This is, to put it quite simply, that some connected speech processes will show social differentiation in a speech community.

TABLE 3

Connected speech processes in Durham (from Kerswill, 1987: 44)

(1) CSP present in Durham, absent in RP:

Regressive voicing assimiliation:

like [g] bairns;
like [g] me;
each [dʒ] deputy;
this [z] village;
scraped [d] down;
what's [dz] gone in, man?
good chap [b], Jack

(2) CSP present in RP, absent in Durham:

Assimilation of place of articulation:

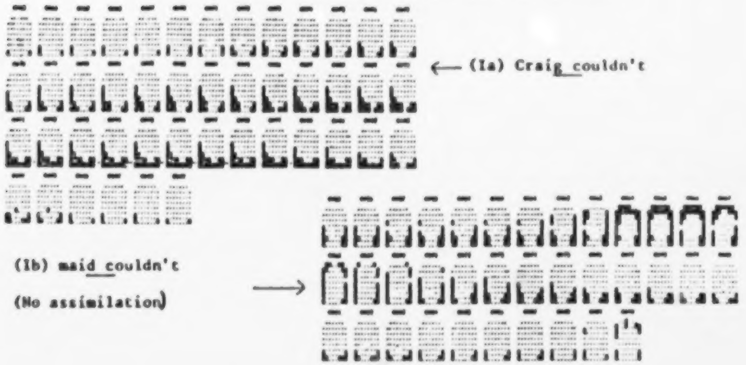
that pen [ðæʔt pɛn] -> [ðæʔppɛn]
that cup [ðæʔt kʌʔp) -> [ðæʔkkʌʔp]
good pen [gʊd pɛn] -> [gʊbpɛn]
good car [gʊd kɑː] -> [gʊgkɑː]

In the Cambridge project, we are looking at a range of processes, particularly place assimilation, *l*-vocalisation, syllable deletion and palatalisation. We are doing this combining the techniques of sociolinguistics with those of experimental phonetics. We are looking at natural speech from a sample of speakers differentiated by social class, sex and age. At the same time as looking at social differentiation, we are also looking at the effects of speech style, particularly speaking rate, as well as the more usual style parameter of formality. (Various aspects of the project, along with some of our results, are reported in Kerswill, 1985b; Wright, 1986; Nolan and Kerswill, 1988; Wright and Kerswill, 1989.)

An important part of our hypothesis is that some CSPs will behave in a way comparable with 'ordinary' sociolinguistic variables. We also hypothesise that some of these sociolinguistically salient CSPs will tend towards articulatory discreteness: that is, they will apply in an all-or-nothing way. They will, in other words, be beginning to show the characteristics of fossilisation and subsequent phonologisation. On the other hand, non-salient CSPs will be more purely phonetically, or naturally, motivated, and will be directly sensitive to speaking rate changes. As such, we can expect them to be phonetically gradual in their application. We can, then, expect to find varying degrees of partial deletions, partial assimilations, residual articulatory gestures, etc. This notion of articulatory gradualness would seem to be especially relevant to one particular favourite sociolinguistic variable: that of final *t* or *d* deletion; yet gradualness does not appear to have been considered in the context of these variables.

We need, then, to be able to identify this articulatory gradualness. To do this, we carried out an electropalatographic study of assimilations. *Electropalatography* (EPG) is a technique which allows the dynamic contact of the tongue against the roof of the mouth to be recorded. The subject wears a specially-made acryllic palate in which are embedded 62 electrodes. A computer records the contact of the tongue with these electrodes. Fig. 2 shows some typical EPG output. Each 'palatogram' shows the degree of lingual contact with the palate during a particular 10 ms window; the top row of dots represents electrodes situated along the alveolar ridge, the bottom row those at the junction between the hard and soft palates. Fig. 2 (overleaf) shows the tongue contacts at the word boundaries in utterances where there is a (potential) assimilation of a final *d* to an initial *k*, (Ib, IIb, IIIb) together with 'control' utterances with 'underlying' final *g* (Ia, IIa, IIIa). Details of the analysis will be given below; but suffice it to say that there is clear evidence here of articulatorily gradualness, shown by the progression from a complete lack of assimilation (Ib), through a partial assimilation (IIb), to a complete assimilation (IIIb).

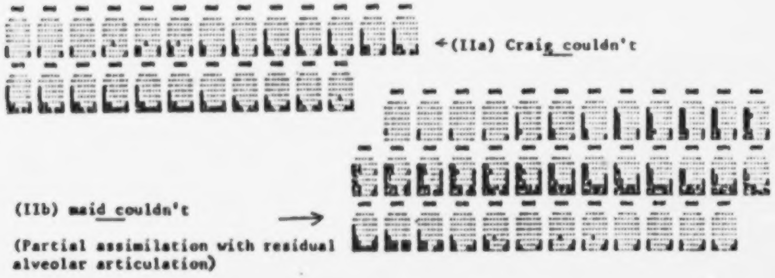
FIGURE 2
Palatograms showing degrees of assimilation



(Ia) Craig couldn't

(Ib) maid couldn't

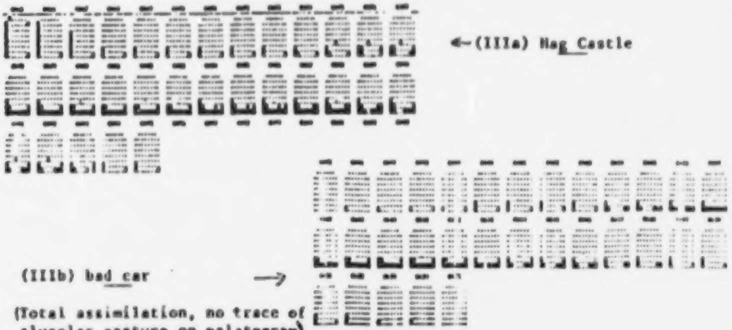
(No assimilation)



(IIa) Craig couldn't

(IIb) maid couldn't

(Partial assimilation with residual alveolar articulation)



(IIIa) Hag Castle

(IIIb) bad car

(Total assimilation, no trace of alveolar gesture on palatogram)

From the point of view of transcription, the relationship between articulation and the percept is an extremely important one. This is not only true with gradual processes such as assimilation, but also in transcription generally. To illustrate this, we can take the potential minimal pair shown below. Do these ever merge, as suggested by the transcription given, or will there always be some articulatory or auditory difference?

fang collector, fan collector -> [fæŋ kəlɛktə]

The question is: does perception operate phoneme-categorially, and classify intermediate forms decisively as (in this example) *fan* or *fang*; and if so, can we talk in terms of a perceptual boundary lying on a putative continuum of alveolar loss? How would this affect a phonetician's attempt at transcribing a potential assimilation? The relationship between articulation and perception is something that our experiment has tried to elucidate.

Finally, before we consider the experiment, we shall raise an issue that is well known, but still not sufficiently discussed: this is the likelihood that a segmental transcription predisposes the phonetician to transcribe a series of discrete articulations, whereas we know that articulations blend and overlap in a complex way. It is true that a transcription can record double articulations, partially overlapping articulations, and even the spread of a feature, such as nasalisation, over more than one segment. Despite this, the segments do get transcribed in sequence. Moreover, and this is important from our point of view, the segments tend to get transcribed in an all-or-nothing way. All this predisposes the transcriber to hear a series of discrete, completely articulated segments.

3. The experiment

This experiment explores the relationship between auditory phonetic transcription and some aspects of articulatory fact by comparing transcriptions of potential assimilations with EPG records of the same tokens.

On the basis of earlier experiments, we decided to use as categories three 'degrees' of assimilation. These are associated with three different scores, and correspond to the categories shown in Fig. 2, above. The categories, or *EPG conditions*, can be more explicitly defined as follows:

1. *Full alveolar*: the EPG record shows a complete alveolar closure at some point during the articulation.

2. *Partial alveolar* (residual alveolar gesture): the record shows more lateral and/or alveolar contact than the non-assimilating environment, but nonetheless shows no complete closure at any point during the articulation.

3. *Zero alveolar* (complete assimilation): the record is either identical with the non-assimilating environment, or else shows less lateral and/or alveolar contact than it.

For reasons which will become clear below, we added a fourth EPG condition:

4. *Non-alveolar* (underlying velar or bilabial).

We then made a list of sentences containing possible word-final assimilations of /d/ to a following velar or bilabial, together with 'control' sentences with underlying velars and bilabials. (We did not include /t/: final /t/ is normally realised as a pure glottal stop in many varieties of English, particularly preconsonantally, as in these examples. Our parallel study of final /n/ will be reported elsewhere, Wright and Kerswill, in prep.) The assimilation 'sites' and their controls are given below:

	Assimilation site	Control
d+k	road collapsed	rogue collapsed
d+k	Byrd concert	Berg concert
d+k	fad catch	fag catch
d+g	did gardens	dig gardens

d+g	bed girls	beg girls
d+g	lead got	leg got
d+m	bride must	bribe must

We got a phonetician to make EPG and audio recordings of these sentences. The ones with underlying alveolars were recorded with each of the three 'degrees' of assimilation: with full alveolar articulation, with partial alveolar closure, and with no alveolar closure. The control utterances were also recorded. This gave us tokens of our four 'EPG conditions' - three underlyingly alveolar, one underlyingly velar or bilabial. In all the tokens, any hint of an audible release was avoided. In order to partially guard against any unrepresentativeness in the production by the phonetician, we compared his EPG records with those produced by a linguistically naïve speaker in an earlier experiment. On the basis of this comparison, we picked out the 'best' tokens of each category for use in the listening test.

The tokens were transferred to a test tape in such a way that the 'control' member of each sentence pair occurred four times and each of the three degrees of assimilation for the underlying alveolars occurred twice each. This gave us a tape on which one-third of the tokens were control items. They were ordered such that identical sentences and 'articulation types' were not adjacent. Thirteen other phoneticians then acted as subjects. Their task was to provide the following:

- a narrow transcription (of preceding vowel and consonant assimilation site)
- lexical identification (judgment of underlying final /g,b/ vs. underlying /d/)
- rating of words judged to end in an alveolar as having:

full alveolar contact,
partial alveolar contact, or
zero alveolar contact

In this way, we hoped to be able to see what criteria, if any, the transcribers used in deciding between the various degrees of assimilation and the non-alveolar control environments.

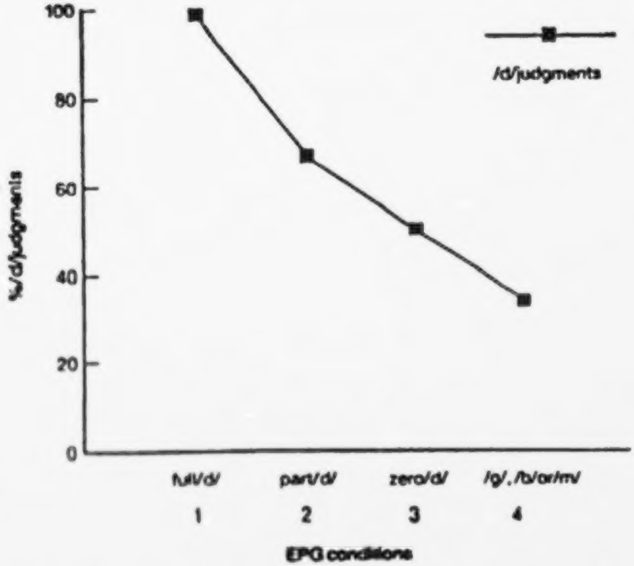
3.2 Articulatory gradualness reflected in identifications

The results of the identification part of the task were as shown in Fig. 3, which gives the phoneticians' judgments of the tokens as underlyingly alveolar. Tokens which were articulated with a complete alveolar closure ('EPG condition 1') were almost consistently identified as alveolar. The percentage of alveolar identifications rapidly drops across the other three EPG conditions - partial alveolar, completely assimilated ('zero') alveolar and underlyingly velar/bilabial.

As we would expect, a good deal of 'alveolarity' seems also to be cued by the auditory impression made by the partially assimilated tokens (condition 2). However, perhaps the most interesting results concern conditions 3 and 4, both of which show substantial alveolar scores. Before attempting to interpret the scores for these two conditions, we should first ask why any of the condition 4 tokens should be rated as alveolar at all. Three factors should be noted: (1) any 'error' will raise, not lower the score; (2) we can expect listeners to try to 'hear' alveolarity even when there is none; and (3) due to redundancy, some phonetic indeterminacy is tolerated in natural speech; here, in the absence of redundancy, the phonetic indeterminacy becomes critical.

Condition 3 tokens are identified as alveolar more frequently than condition 4 tokens, which, according to the EPG record, are completely assimilated. This evidence suggests that there is, in many if not all of the condition 3 tokens, some kind of articulatory 'residue' which is having acoustic consequences without leaving a trace in the EPG record. The question then arises: what is the nature of this acoustic cue, and how do phoneticians set about exploiting it in a phonetic transcription?

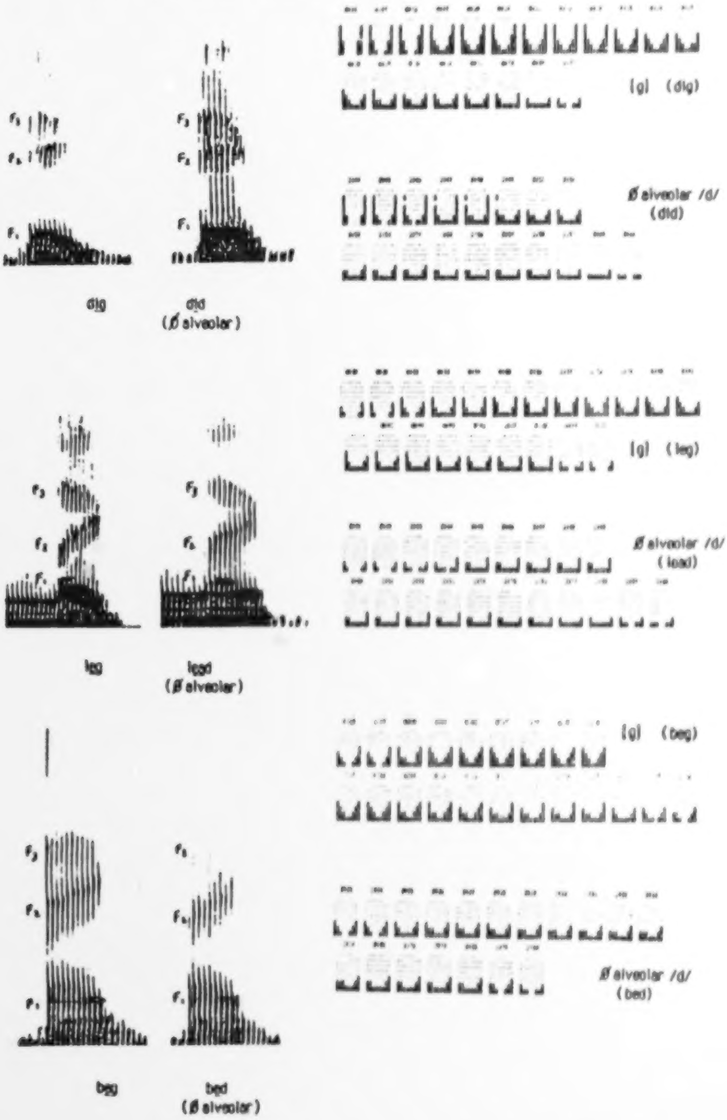
FIGURE 3
Percentage alveolar identifications for four EPG conditions



We will look first to see if EPG can give us any indications as to what these cues are. Remember that a residual alveolar contact shows up as lateral contact and partial alveolar contact. However, looking at some of the tokens that we originally classed as 'completely assimilated', we note something peculiar. This shown in Fig. 4 (overleaf). Note how, in these pairs, it is the assimilated alveolar that has the lesser lateral contact and the more retracted velar articulation. This is intuitively unexpected. But look at the identification scores for these three items (Table 4, overleaf).

For two of these three pairs (*lead/leg* and *bed/beg*), the difference between the scores for the two types is very much greater than for all the pairs taken together; as Table 4 shows, this is not true of any other single pair. There is, therefore, something differentiating these pairs rather

FIGURE 4
Palatograms and spectrograms showing alveolars and velars



clearly. This is evidently not alveolar contact. The EPG patterns of Fig. 4 can in fact be taken as evidence of a residual tongue body configuration appropriate for an alveolar: as the tongue tip moves up towards the alveolar ridge, the blade and pre-dorsum become concave; this reduces the amount of lateral contact in the pre-velar area. At the same time, this tongue shape will cause the velar contact itself to be more retracted. Some support for this interpretation is provided by spectrographic data

TABLE 4
Identification scores for individual tokens - EPG conditions 3 and 4

EPG CONDITION 3

Identifications as:

	Alveolar	*Non-alveolar*	%alveolar
did/dig	*16*	*12*	57
lead/leg	*15*	*13*	54
bed/beg	*16*	*12*	57
road/rogue	*16*	*12*	57
Byrd/Berg	*12*	*16*	43
fad/fag	*8*	*20*	29
bride/bribe	*15*	*13*	54

EPG CONDITION 4

Identifications as:

	Alveolar	*Non-alveolar*	%alveolar	Difference between conditions 3 & 4
did/dig	*25*	*31*	45	12
lead/leg	*8*	*48*	14	40
bed/beg	*12*	*44*	21	36
road/rogue	*28*	*28*	50	7
Byrd/Berg	*27*	*29*	48	-5
fad/fag	*8*	*48*	14	15
bride/bribe	*24*	*32*	43	11

for at least one of these pairs: in Fig. 4, the locus of F2 and F3 is higher for *dig* than for *did*, which suggests velar and alveolar offsets, respectively. This lingual configuration may in fact be heard as 'alveolarity'. This is the reason why the more retracted articulation is heard as more alveolar: it is the overall configuration of the tongue that has the acoustic consequences.

3.3 Transcription strategies - a mixed bag

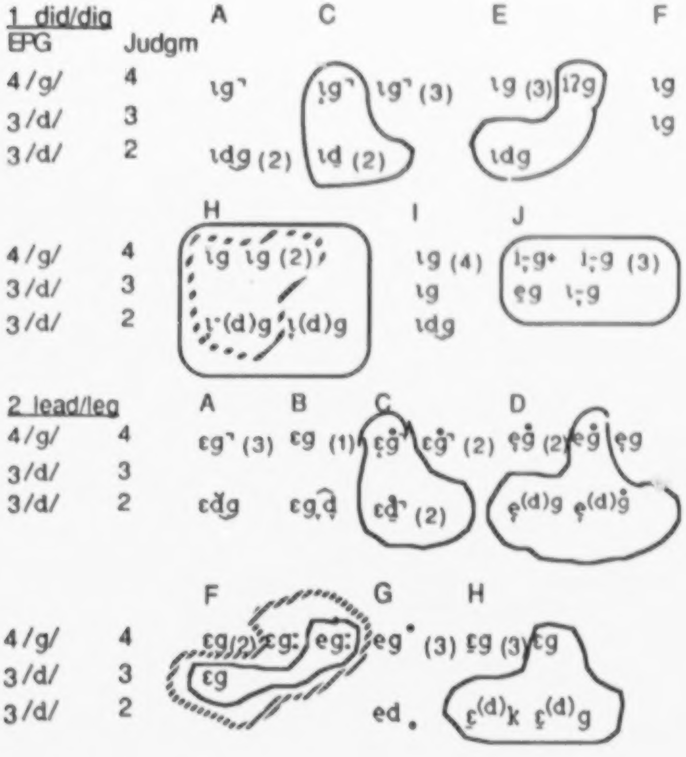
EPG gives us, then, a clue as to the articulatory correlates of assimilation. In another paper (Wright & Kerswill, 1989), we have argued that this data suggests that there may be no such thing as 'complete' assimilation: there is always some articulatory 'residue' in 'maximally' assimilated items. However, here we shall look in some detail at how transcribers set about rationalising and reducing to symbols the differences they have heard. Table 5 shows the the transcriptions of condition 4 and condition 3 tokens of the three pairs just mentioned. (We have included only those transcriptions where (a) condition 4 tokens were correctly identified as velars, *and* (b) condition 3 tokens were correctly identified as alveolars and judged as having either partial or zero (but not full) alveolar articulation.) A striking overall pattern is the high frequency with which condition 3 is 'heard' as a partial alveolar rather than as the 'correct' zero alveolar. This should not surprise us, since once transcribers have decided they are listening to an alveolar, they will presumably try to indicate some sort of alveolarity in the transcription.

It is more interesting, however, to try to establish the strategies transcribers use to differentiate the velar and the alveolar tokens, and then to try to match these with the acoustic and articulatory data. An inspection of Table 5 shows there is much individual variation. However, three strategies seem to recur: these involve marking differences in vowel or consonant length, differences in vowel quality, and consonantal differences. We will discuss these in turn.

TABLE 5
Transcriptions of tokens of three pairs of items

NOTE: only those transcriptions have been included where (a) condition 4 tokens were correctly identified as velars, and (b) condition 3 tokens were correctly identified as alveolars and judged as having either zero ('3') or partial ('2') (but not full) alveolar articulation.

Item: Transcriber:

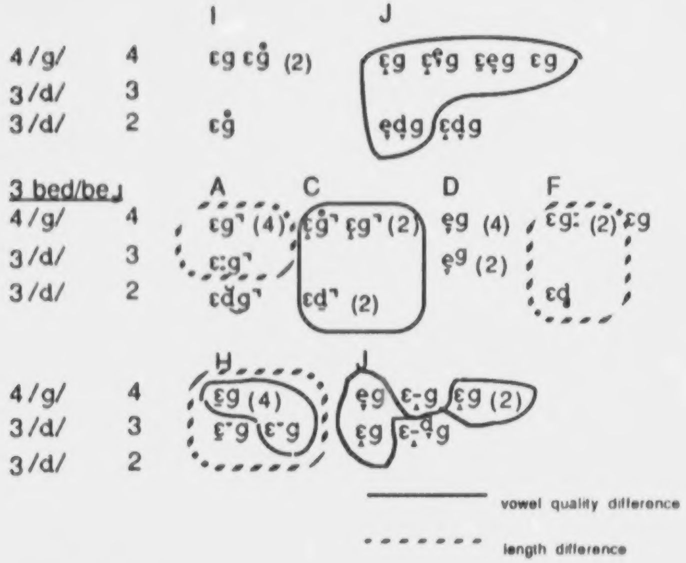


1 did/dig EPG	Judgm	A	C	E	F
4 /g/	4	ɪg˺	ɪ̤g˺ ɪg˺ (3)	ɪg (3) ɪʔg	ɪg
3 /d/	3				ɪg
3 /d/	2	ɪd̤g (2)	ɪd̤ (2)	ɪdg	

		H	I	J
4 /g/	4	ɪg ɪg (2)	ɪg (4)	ɪ̤ːg˖ ɪ̤ːg (3)
3 /d/	3		ɪg	e̤g ɪ̤ːg
3 /d/	2	ɪ̤ˑ(d)g ɪ̤(d)g	ɪd̤g	

2 lead/leg		A	B	C	D
4 /g/	4	εg˺ (3)	εg (1)	ε̤ġ˺ εġ˺ (2)	e̤ġ (2) eġ e̤g
3 /d/	3				
3 /d/	2	εd̤̆g	εg͡d̥	εd̤̊˺ (2)	e̤$^{(d)}$g e̤$^{(d)}$ġ

		F	G	H
4 /g/	4	εg (2) εgː eːgː	eg˚ (3)	ɛ̰g (3) ɛg
3 /d/	3	εg		
3 /d/	2		ed̥	ɛ̰$^{(d)}$k ɛ̰$^{(d)}$g



Vowel and consonant length

In five cases (enclosed in the table by a broken line), transcribers mark length differences. In three of these cases, the alveolar is heard as being preceded by a longer vowel, while in the other two the velar is given a longer consonant closure. Surprisingly, there is no evidence at all of longer vowels in *bed* and *did* than in their velar counterparts (see Fig. 4); yet for *lead*, whose vowel is measurably longer, no transcribers indicate this length. Consonant length differences (enclosed by a continuous line) can perhaps be seen as the other side of the same coin: a consonant after a durationally short vowel may be auditorily longer than after a longer vowel. If this is so, it is no less 'correct' to indicate a long consonant than to indicate a short vowel.

All five cases of length difference seem, then, to point in the same direction. However, there is disagreement between the transcribers as to where this length resides. And where there is a clear vowel duration difference, this is apparently not heard as such; conversely, when there is no measurable difference in vowel duration, some transcribers seem to want to mark one. Whether or not there are consonant duration differences will have to await further spectrographic analysis. But for the moment, how can we explain the evident mismatch between measurable vowel durations and the transcriptions? As linguists, phoneticians 'know' about allophonic vowel duration differences, and it may be that they are trying to 'hear' such a difference - even though none is predicted phonologically (both /g/ and /d/ are voiced). Indicating length may be a more or less conscious attempt to rationalise a difference they can hear, using the limited resources of the IPA - one of which is to mark length. Alternatively, the percept of a length difference may be the psychoacoustic correlate of a consistent phonetic difference. As such, the percept is 'real' in the sense that it is not the consequence of an attempt to mark a difference willy-nilly, as in the case of the first explanation. Both explanations may have an element of truth in them: the fact that the differences marked by the transcribers are consistent with each other suggests a 'real' perceptual difference, while the disagreement as to where the difference lies suggests ad hoc attempts to indicate it using the transcription resources available.

Vowel quality differences

In twelve cases, we find vowel quality differences. In nine, the vowel before the velar is heard as closer than that before the alveolar; in only one case is the opposite true. Inspection of the spectrograms in Fig. 4 does not reveal any decisive differences; however, 'reading' vowel quality from the rapidly changing patterns on a spectrogram is notoriously difficult. There is obviously considerable agreement among the transcribers; even so, we must question the validity of their transcriptions because of the influence of their assumed prior 'knowledge' that closer allophones of vowels occur before velars. To test this source of error, we would need to carry out perception tasks using synthetic stimuli, or using edited natural stimuli from the which vowel offsets have been removed. However, the strength of the agreement certainly suggests the preservation of allophonic height differences even after the final consonant has been apparently assimilated.

Consonant differences

In most of the cases, the transcribers note consonantal differences. This is particularly true, of course, where the transcriber has judged the alveolar as having a 'partial' articulation. There is a multiplicity of transcription strategies, suggesting that it is in the transcription of the consonant that the IPA itself fares worst. Strategies include using:

'no release' diacritic
'voicelessness' diacritic
'double articulation' diacritic
'retraction' diacritic
'fronting' diacritic
'length' diacritic
'shortness' diacritic
'lowering' diacritic
parentheses
superscripts

Some of these can be interpreted as representing the same intention on the part of the transcriber, though some can be taken simply to mean uncertainty (especially parentheses and superscripts). It is quite clear, however, that, unlike in the case of the vowels, the transcribers are explicitly aiming to represent *articulation* rather than, say, an abstract auditory parameter that might be labelled 'alveolarity'. The success of their enterprise will depend at the very least on (a) their ability to discriminate without being influenced by their phonological knowledge; (b) their experience with transcription; and (c) their knowledge of articulatory phonetics. To this must, of course, be added their degree of commitment to the task.

In representing what they hear for the consonants, the transcribers are constrained by the segmental nature of the IPA, and the relative difficulty of indicating phonetic features which change gradually over time and which are spread over more than one 'segment'.

4. Discussion

We think the identification of these tokens as alveolar or otherwise involves an extremely complex set of factors. Firstly, the listener must have knowledge of the auditory effects of different tongue gestures, including the 'residual' ones we are hypothesising. Secondly, as is well known, vowel quality and vowel length vary in different consonantal contexts; it is likely that these differerences remain even after so-called assimilation has taken place, and continue to cue alveolarity. Lastly, an important part of these allophonic differences in vowels is that, in spite of certain universal tendencies, they are to a great extent dialect-specific, and the listener needs to have knowledge of the dialect (in this case, the speaker had mild south Yorkshire accent), and even knowledge of the speaker himself, to be able to unravel all these effects in such a way as to utilise them.

Transcription is a messy thing. For some people in this study, it is a way of representing a sequence of segments which are either articulatorily complete or non-existent - as some of the transcriptions show. Others seem more willing to allow incomplete or overlapping segments, but are still bound by the notion of articulatory segments. Yet others transcribe vowel quality differences. But we still don't know whether the vowel differences are due to residual articulatory gestures, or whether they are phonologically-determined, accent-specific allophonic differences that remain even where there is no residual articulatory gesture. In some cases, the transcribers could even, consciously or unconsciously, be tuning in to formant transitions which are not normally considered part of vowel quality and which are certainly not considered part of a phonological analysis.

To sum up, the problem lies in an inherent multi-layered ambiguity in the task of transcription itself. First, transcription is either meant to represent articulations, or it is meant to represent auditory impressions. Second, it either represents discrete segments, in which case it presupposes a prior phonological analysis, or it represents a continuously varying acoustic signal. Lastly, the continuous nature of the acoustic signal is either the result of pure, universal coarticulation or it is the result of accent-specific allophonic and sandhi rules. The snag is, all these things are true to different degrees, and unfortunately transcribers will put the boundary between each of the pairs of opposites in different places. This is what we meant when we said at the outset that transcribing without any kind of theory is a dangerous thing: we simply do not know exactly what each individual is doing, and consequently we cannot interpret precisely what they write down.

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CURRENT LANGUAGE PLANNING AND POLICY IN CATALONIA*

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Introduction

This paper will seek to assess the present status and position of the Catalan language and to examine the language planning activities being carried out in Catalonia to promote this language. A brief outline of the historical development of the Catalan language and a description of the situation in which it finds itself today in Catalonia will be given. The likely future for Catalan will be examined, particularly its relationship to Castilian, the main language of the Spanish state.

Whilst Catalan is spoken in some form and by a varying degree of the population as far afield as from the Spanish province of Alicante in the south to the Roussillon area of southern France in the north, including the Balearic Islands and even a small part of Sardinia, this paper will concentrate on Catalonia, the region of which Barcelona is the capital. This area, with a population of some six millions, has been an Autonomous Region since 1979 under the new Spanish constitution, and has therefore been in a position to encourage and promote the local and widely-used language, Catalan.

The official activities in Catalonia to extend the knowledge and use of Catalan have been consciously planned by the local government, and in this sense constitute a form of Language Planning. The term Language Planning will, however, be used throughout in a broad gen-

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eral sense, rather than the more specific sense of Language Planning theory developed over the last two decades.[1] The Language Planners in Catalonia do not claim to follow any deliberate Language Planning model, although they are clearly aware of the examples being implemented in many parts of the world. They are well-informed of the literature on the subject, and this may at least indirectly influence the way they have organised their planning. In particular, it would seem that they have kept the classic division between status planning and corpus planning (see Kloss 1968 and Cobarrubias 1983).

The relationship between policy makers and planners, politics and implementation is a very close one in Catalonia and one which, it will be argued, appears to guide the language planners' overall objective of total bilingualism, rather than one of, for example, eventual Catalan dominance.

The current policy being pursued is to re-introduce Catalan as an official language, alongside Castilian, at all levels within Catalonia. Whilst Catalan is a widely spoken language with a respectable written literature, opposition to this kind of promotion might have been anticipated: an obstacle to communications compared with Castilian which is so much more widely spoken, including internationally; or the expense of changing the balance of the main means of linguistic communication, being just two such possible reactions. In the event, the degree of consensus in Catalonia to the present policies has been strikingly high, reflecting, it would seem, the prevailing attitude that the language is an important symbol of Catalan ethnic and historical identity.[2]

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[1] See, for example, Haugen (1966), Rubin & Jernudd (1971), and Cobarrubias & Fishman (1983).

[2] There was no public opposition voiced during the debates on either the Autonomy statute or the Language Normalisation Law, but it must be noted that those most likely to object to the imposition of Catalan, i.e. the non-Catalan immigrant population, are by definition the least vocal or articulate. In March 1981 a group of about a hundred intellectuals, largely non-Catalan, published a leaflet against what they perceived as potential discrimination because of an over-zealous Catalanization programme. This

The historical development of Catalan

The Catalan language is a romance language which developed from around the tenth century and was the language of a highly influential seaborne empire in the Middle Ages. During this period it was a language of important literary output with all the functions traditionally designated as both High and Low (Ferguson 1959). As Catalonia became politically dominated by Castille, however, the Catalan language suffered persecution and repression and declined to being the L variety in a diglossic situation with Castilian (see Ferrer i Gerones 1985).

This situation began to change again in the nineteenth century. A re-birth in public and cultural use of Catalan came with the appropriately named *Renaixença*. This was, above all, a literary revival, seeing also the appearance of the first newspapers in Catalan and in general an upsurge in nationalistic cultural pride.[3] During the early part of the Twentieth century Catalan saw various periods of active revival, coinciding always with a progressive government in Madrid. During the period leading up to the Civil War the Catalans were allowed much in the way of self-government and Catalan became the dominant language once more. It was also during the early part of this century that much work took place on the codification of Catalan, including the publication of a definitive grammar and dictionary. Newspapers were published in Catalan and during the Thirties some schooling was in Catalan.

The Civil War and the Franco regime brought a repressive end to the use of Catalan which was proscribed for many years during this tightly centrist dictatorship. The new post-Franco democracy has allowed the language to begin to return to its former role, and now over a

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does not seem to have left much of a dent in the language programme. Another source of criticism, or only lukewarm support, has been from the (non-Catalan) Socialist Party.

[3] For a detailed discussion of the *Renaixença*, see (Vallverdu 1970: Chapter III).

decade after Franco's death it is interesting to see what progress has been made.

The use of Catalan today

Today Catalan has indeed returned to being a widely heard and used language in Catalonia, but its place as the dominant language is not so clear as would have been the case a century ago. The two main reasons why Catalan must now fight against Castilian to survive as a language suitable for all situations are first, the present demographic profile, and second, modern communications and technology.

i. Immigration

After the Civil War Catalonia experienced an enormous influx of migration, mostly from Southern Spain. The native Catalan population was therefore greatly diluted in some areas. It is estimated that almost half Catalonia's population is non-Catalan or only first generation Catalan (see e.g. Reixach 1985 and CIDC 1987a). From the point of view of learning the Catalan language, this is made worse by the fact that the majority of this immigration settled in and around Barcelona. In some areas of Greater Barcelona as many as 90% of the population are non-Catalan. Add to this the fact that these migrants arrived in Catalonia during the Franco years, years of great repression of Catalan, and the likelihood of the new arrivals learning Catalan was even less. These immigrants were also often illiterate or with very limited education. They, therefore, had very different learning needs from much of the indigenous population and provided a further challenge for the language planners.

However, immigrants to Barcelona have quickly appreciated the unusual position of Catalan: that although it is a minority and historically much-persecuted language, Catalan remains, within the boundaries where it is spoken, the community's high prestige language. Catalan is the language of the middle classes, of the boardroom, of the employed. It is perceived as the language of social mobility and therefore there have been positive attitudes to learning at least to understand Catalan, and above all to non-Catalans allowing their children to learn Catalan.[4]

The most recent census in 1986 shows as many as 90% of the population of Catalonia claiming to understand Catalan, although considerably fewer speak it, and far fewer read and write it (CIDC 1987b). This is an improvement even on the comprehension figures reported in 1981 (and compared in the 1986 census, CIDC 1987b) and should be an optimistic sign for the Language Planners.

ii. Modern Communications and Catalan

The promotion of Catalan, in particular amongst the non-Catalan population of Catalonia also has to contend with modern mass media and communications, and easy international travel. In all these situations Castilian, or other world languages, are immediately available. Catalan has had to learn to compete for space in radio, T.V. and the press.

There is considerably more Catalan now heard in radio and T.V. broadcasts, particularly with the opening of a third state T.V. channel entirely in Catalan. Importantly, too, this does not just signify a quantitative improvement, but also a qualitative one, with Catalan being used now in all spheres of life and differing programmes, and not just, as in the past, for folkloric and chatty local events, with 'serious' discussion remaining in Castilian.

There are currently three daily newspapers published in Catalan, two based in Barcelona, and one in Gerona. All are serious publications with respectable circulations. Similarly great strides have been made in book publishing in Catalan, with the number of titles in Catalan over the last few years rocketing, helped by the fact that Barcelona has always been an important publishing centre. However, the fact remains that the market for Castilian, a world language stretching across continents, is commercially far more attractive than for Catalan.

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[4] See, for example, the surveys carried out by Sole (1983) and Strubell & Romani (1986).

It can be seen that Catalan is making progress in its bid to re-establish itself as a major national language in Catalonia. Catalan theatre is popular and active, and there is an emerging Catalan film industry. Public notices are now found in Catalan, as well as, or even instead of, Castilian. All local administration must normally now take place in Catalan, although Catalonia's official bilingualism means that the use of Castilian can be insisted upon by any individual. Shop signs, menus, train timetables, bank cheque books, and much other printed material is increasingly available, sometimes exclusively, in Catalan. Nonetheless, Catalan continues to face an uphill battle to equal or replace the widespread use of Castilian.

The legal framework for language planning in Catalonia

Language Planning activities in Catalonia result from the various legal decrees and laws that mark the commitment to the promotion of Catalan by the local government, the *Generalitat*, and, to a lesser extent, by the Spanish state.

Spain's present constitution recognises the existence of languages other than Castilian and their right to co-officialdom within the territories in which they are found. (Spanish Constitution 1978) The Catalan Statute of Autonomy extends this recognition to spell out the rights and duties of the population of Catalonia to know both languages and to use either (Catalan Autonomy Statute 1979). It also guarantees that the Generalitat will 'create suitable conditions so that full equality between the two [languages] can be achieved' within Catalonia. In 1983 the Generalitat passed the Law of Linguistic Normalisation which further clarifies and explains these rights and gives the framework for the language planning activities.

Under the Department of Culture, the Generalitat established a Directorate General of Language policy, with three sub-branches. One has responsibility for promoting the official use of the language; one has the responsibility for developing terminologies, providing translation services and Catalan classes for adults; and one is a

Sociolinguistics Institute responsible for evaluating the work of the language planners, for collating relevant literature and for analysing the basic theoretical concepts of the language questions.

The first of these sections is the one primarily responsible for the *Programa de Normalització Lingüística*, the Language Normalisation Programme. This programme is the central point in the present language planning activity, with its campaigns to promote the use of Catalan in all public and official walks of life. The term 'normalisation' is a significant one in this context as it represents the objectives of the planners to return to Catalan its 'normal' functions as a major vehicle of communication. This section has set up a region-wide network of offices working with each town council, and mounts many campaigns and produces prolific literature. In 1986 a Catalan language Congress was held in many different venues in Catalonia which brought together not only those working to promote Catalan within Catalonia but also many international experts.

Catalan and the education system

Perhaps, however, the most interesting area of language planning lies with the Department of Education, which, surprisingly, is totally separate from the Directorate General. This department is responsible for the teaching of Catalan in the schools and colleges, but not to adults, who are served by the Directorate General.

Since 1979 much progress has been made in introducing more Catalan in the schools (see Arnau & Boada 1986), first by making some teaching of Catalan compulsory, and gradually by encouraging more and more schools to use Catalan as the medium of instruction in one or more subjects. It is now a compulsory minimum to teach at least three hours a week of Catalan plus one subject in Catalan. However, the reverse is also true for Castilian, which is important if more schools become almost entirely Catalan speaking. The equal legal provision for the two languages underlies the fact that in all these policies it can be seen that the raising of Catalan only to an equal footing with Castilian is seen as the ultimate objective. The law guarantees the right of children to begin their studies in their mother tongue, whether it be Castilian or Catalan. It is clear that the present legal position does not seek to impose Catalan through the schools, only to protect those who wish to introduce it.

There are, however, many problems for the introduction of Catalan in the schools, the most significant being the lack of suitably trained teachers and of materials. Many teachers are from outside Catalonia, particularly in the high-immigration areas, and even amongst those who are native Catalan speakers, many have not been taught or trained in Catalan. Even of those who are now competent in speaking and writing Catalan, many have not been trained to be linguistically aware of the needs of their pupils when teaching their own subject, such as, for example, maths, through a different linguistic medium. There is also the problem that there is a significant private sector in the education system. Since the language planning activities are state initiated, this makes coherence difficult. More importantly, the private schools tend to have larger numbers of native Catalan pupils and teachers, thereby creaming these people off and making integration difficult and thus losing the chance of creating more Catalan environments for children from non-Catalan families. It serves too to accentuate the social divide between the middle classes who are Catalan speaking and the migrant workers who are Castilian speaking.

To help overcome these problems, the planners have introduced immersion programmes, based to some extent on North American models. These schools, which exist only at primary level, are situated in areas of high non-Catalan populations and provide education entirely in Catalan to help immerse the child from an early age in Catalan culture. These schools only began to operate in 1983, and it is too early to see how successful they are. (see Arenas 1986 for some early evaluations) They are entirely optional and work only with the cooperation of the teachers and parents. They do, however, have their critics who believe that teachers are not yet sufficiently trained to staff these schools and that the cooperation from the community is too apathetic to be successful. They are concerned too whether children learn more slowly when not taught in their mother tongue. There is an anxiety, also, about whether the level of funding for these programmes, including in-service training for teachers, is adequate.

The eventual aim, and legal duty, of the Catalan education system is that children will finish their basic studies at 14 bilingual in Castilian and Catalan, with equal facility in both. This is certainly not the case yet, as can be seen from various surveys that have been carried out (e.g. SEDEC 1982 and Departament d'Ensenyament 1984), but one must note that the changes are in their early stages. It is evident from these surveys that children make far faster progress in Castilian than Catalan given the same context, and that for rapid progress in Catalan, a very Catalan environment is needed. For advances in Castilian the predominance of the Castilian media makes this simpler.

What is Catalan's future?

The immediate future for Catalan seems promising, and certainly as a family language, at least, there seems no doubt that it will survive. Whether it acquires and sustains a status as a genuinely equal language with Castilian depends above all on whether the large non-Catalan population can be integrated into Catalonia, and whether they -- or their children at least -- can be taught the language. It depends too on Catalan's effectiveness in competing with Castilian and with other world languages.

It also depends to some extent on Spain maintaining and developing its newly-defined character as a multilingual state. This is moreover fairly limited, insofar as regional languages are only encouraged and officially recognised within the boundaries where they are spoken. No attempt is made, say, to teach Catalan in Seville; or Basque in Catalonia. It is not clear that a Catalan or Basque member of the Spanish parliament would be in their right to speak in Catalan or Basque.

The question arises of how realistic it is to promote Catalan beyond the boundaries of Catalonia. Communications certainly would be simpler in the first instance if Castilian was the only official language, both within the rest of Spain and with much of the outside world. The promotion of Catalan is above all the response to a sense of ethnic identity. Nonetheless, if this 'normalising' process is to be successful, it could be argued that it would not be enough only to aim to make Catalan equal to Castilian in all domains within Catalonia. As a world language and the language of the Spanish state, not to mention the first language of many who live in Catalonia, Castilian will always remain the dominant partner in such a form of bilingualism. If Catalan is not to revert to being only the language of the family and other L domains, it seems logical that it should have to be promoted as the SOLE official language of Catalonia.

This, however, would involve unsavoury discriminatory policies and the sort of planning decisions that lead to unacceptable interventions in people's lives. It would also mean the channelling of limited resources to this end in ways which might be to the detriment of those most in need. For example, it is hard to defend money being spent on the devising of Catalan materials and the training of Immersion Programme teachers, if this should lead to less money being spent on the building and basic equipping of much-needed schools in the first place.

The present policy-makers and language planners have clearly recognised that any overall aim towards Catalan monolingualism would be potentially racist and politically and morally unacceptable. There are those who do not agree with them, but it is hard to see how one can combine enthusiasm and optimism for Catalan's future as a major language with ideologically acceptable levels of social planning.[5]

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[5] There are radical groups who do not believe that the pace of Catalanization is quick enough, and who are intolerant of the non-Catalan population, citing in particular how they were forced to learn Castilian, rather than their mother tongue during the Franco years. Such groups include the CRIDA and the Grups de Defensa de la Llengua. Some leading

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Catalan sociolinguists, however, also worry as to the effectiveness of such an equal bilingualism policy, e.g. Ernesto Sabater:

> 'In a recent publication on bilingualism the following clear opinion of a group of teachers in Quebec appeared, "bilingual education can function in an environment where the mother tongue is not threatened. If the mother tongue is in a position of weakness, then bilingual schooling will deliver the final *coup de grace*." And this is our opinion: Castilian is certainly in no danger of disappearing, but Catalan is, though in the long term [...]' (Sabater 1984: 38).

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CONSTRUCTIVE BELIEFS AND POLITICAL REFERENCE*

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In reply to parliamentary criticism of his attack on the BBC's coverage of the United States' bombing of Libya Norman Tebbit (NT) responded that '... it was not the Chancellor the Duchy of Lancaster (hereafter CODL) who made the complaint but the Chairman of the Conservative party...' (Hansard, November 17th 1986). What is interesting about this choice and use of definite description is that Norman Tebbitt was both the CODL and the Chairman of the Conservative party. While the interaction of this dual set of definite descriptions is interesting in itself, in this paper I want to focus on the general issue of self reference under definite description, and although my arguments attend to NT's reference to himself as the CODL, they would apply equally well to his reference to himself as the Chairman of the Conservative party.

The type of referring form used by NT can be found in many different contexts. My Dean recently used the phrase: it is the function of the Dean to ... etc. The choice of expression is not then particularly remarkable. But one must ask, since both speakers had perfectly acceptable alternatives available to them, i.e., 'it is my function/responsibility', or 'it was me who complained about the BBC', why they chose to refer to themselves in this particular manner.

Intuitively, one might suggest that the distinction is an indicator of 'role' identification, that speakers who have a variety of roles merely wish to specify a particular role relevant to the unfolding discourse. Even if this were the case, it would not explain why it is that such a

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choice is not made on every occasion where it would be possible, suggesting some sociolinguistic/pragmatic grounding in a particular selection: nor does it explain how audiences operate in processing such information, since the effects are significantly different for those audiences who are aware that the definite description refers to the speaker and those who do not have such information (it should be noted that parliamentary debates are broadcast to a radio audience, as well as being made available in a written form in Hansard).

This last claim may seem odd in that it is generally assumed that speakers only use definite descriptions when they can rely on their audience to retrieve the reference. But this assumption must be assessed against a context where a definite description, even if the reference is retrievable, has been employed where the expected form could equally easily have been a self referential pronoun. The puzzle of choice is what I attempt to work out below.

In this paper I want to argue that for both audiences who know that NT=CODL and audiences who do not know NT=CODL difficulties arise, and that the choice of the type of description employed by NT under conditions of self reference may act as a play to deflect specific individual responsibility for certain mentioned behaviours. My concern is 'pragmatic' in so far as I want to explain the effects of a particular contextual choice on the processing of meaning.

Reference and Intentions

'Reference' is a major problem for both linguists and philosophers; it is not my aim here, however, to review the vast literature on referring (for a general perspective see Devitt and Sterelny 1987; on more specific issues Quine 1969; Casteñada, 1968; 1975; Cole, 1979; Boer & Lycan, 1980; 1986). My initial concern is with the distinction made by Donnellan (1966) between the referential and attributive use of definite descriptions. This distinction is based on the principle that speakers' intentions play a major role in distinguishing how an expression is being employed in referring (contra: Russell and Frege). If a speaker uses a definite description referentially then he/she intends the hearer to pick out a specific designated individual. On the other hand, if a speaker uses a definite description attributively the intention is not to designate a specific individual the speaker has in mind, but rather to state something about whoever or whatever is designated by the description.

Finding Smith's dead body, we might draw the conclusion that there has been a murder. We might, in such circumstances, express the view that 'Smith's murderer is insane' without knowing who the murderer was. In this case one would be using the description attributively. If it turns out that Smith died of natural causes then the description fails as an attributive act. On the other hand, if one were to say at a party that 'the tall woman drinking white wine is a teacher', and it turns out that she is in fact drinking water, then it is still possible, where you are correctly understood, that you would have successfully designated a specific individual.

This view of reference offers one possible explanation for the behaviour of Norman Tebbit; at least at a descriptive level. One might argue that when NT refers to himself as the CODL his intention is to use the referring expression attributively. The explicit aim is not to specify a specific individual but whoever is designated by the description. This may seem odd on a common sense view, particularly for those who know that Tebbit is the CODL. But let us concede, for the moment, that if NT can get his audience to think about the individual who is the topic of talk in attributive terms, then any responsibility claims will not be embodied in any single identified person but rather in generic terms relative to whoever or whatever may be the case. The advantage here for any person who is attacked for performing certain actions is that hearers are being directed away from focusing on that person as a specific individual.

Taking this claim as a starting point, and assuming that it is plausible (as far as it goes), how can we explain such an interpretation; and further, how does such a claim take account of the interpretive options available to those audiences who know NT is the CODL as opposed to those audiences who do not? For those who are not aware of the identity equivalence (in the real world), they clearly cannot equate any beliefs or attributes of NT with those of whoever is the CODL, since the assumption is that the identity for them is unknown. In the case of those who are aware of the identity equivalence the problem of explaining any interpretive behaviour is more complicated. Surely they can simply substitute NT for the CODL. For example: NT is speaking, NT is the CODL, therefore the CODL is speaking. I don't want to consider whether such a substitution operates successfully in all environments which might be constructed for the sake of philosophical debate (as in opaque contexts, or contexts of self reference under loss of memory or perceptual trickery: see for example Casteñada, 1968; Quine, 1969); I take it as given that in the real world of discourse that if I know NT=CODL, then it is plausible for me, in constructing models of the actions of NT or the CODL, to treat these identities as intersubstitutable.

If this is true then what is to be gained from using the expression CODL as opposed to some other self referring expression? Perhaps it is the case that the ambiguity inherent in Donnellan's distinction allows NT to 'hedge' (Lakoff, 1972) on any identity claims. Consider (1):

(1) A. Can you fix this needle for me?

B. I'm busy

A. I was only asking if you could fix it.

Most normal speakers of English will recognise that A's first turn has the conventional form of an indirect speech act (Searle, 1975). The problem with such acts is that it is theoretically unclear whether they function as multiple or single units for interpretation (in the above case the first turn could be both question and request). For speakers who use such indirect forms there is an advantage to be had, in that you can always claim of any two interpretations (a) and (b) that only one was intended, the one which suits your purpose. In the case of NT's use of the CODL as a referring phrase under conditions of identity equivalence, similar options seem available. NT can claim either that he was referring to himself, or that he was referring to a role which he just happens to hold (indeed, this claim would fit with Donnellan's suggestion that attributive uses may not refer at all: see Searle, 1979 for a counter view).

There are a number of difficulties here however. Firstly, the use of the referring description has different effects on different audiences, suggesting that whether some expression is referring or attributive is not completely constrained by speakers intentions alone (see Johnson-Laird and Garnham, 1980). The hearer's knowledge of the world in which the expression is used plays a part. Secondly, despite the social role theory, if NT=CODL, and I know this to be true, I know it to be true whether it is explicitly expressed or not. In this case then we need some further pragmatic explanation to account for any rôle interpretation where it emerges via some expressions and not others. Thirdly, why should hearers, as rational agents, believe that the degree or extent of responsibility for actions is in any way mitigated by the use of certain referentially equivalent descriptive phrases?

In order to deal with these questions we need a theory which allows us to take account of the interaction of speaker/hearers' knowledge and beliefs at particular points in the production of interactive discourse. We should not assume that simply because a speaker makes an utterance following certain principles of communication, and with a specific communicative intention (as in the case of Gricean rules for example) that each hearer will necessarily interpret the utterance exactly as intended. Different audiences will react in different ways depending on their own knowledge and beliefs.

I don't think we can ever guarantee the way in which what we say will be interpreted; we depend on conventions rather than hard and fast categorical rules for understanding. Within the conventional expectations of how an utterance would normally be interpreted, however, we can calculate probabilities for different audience responses and select the expression most likely to succeed (see Leech, 1983). Since conventions are generalisations across behaviours, they are abstracted and analysed at specific moments in time relative to our individual concerns and the unfolding interaction. What we need is some way of expressing the conventional interpretation of what NT has said independently of the discourse context, then we can use this as a basis for considering the interpretive and reinterpretive options available to participants within the actual discourse context as it is processed.

Constructive Beliefs

In order to explain how we might deal with speaker/hearer interpretive options I want to consider what Wilks has referred to as a 'constructive theory' of beliefs. Wilks argues that beliefs are processed and understood in terms of specific belief environments. These environments are organisational belief spaces which speaker/hearers employ in achieving understanding. What is particularly important about Wilks' perspective is that the model seems to allow for selective processing, by which I mean that one can select specific environments in which to run arguments; with the obvious consequence that each different environment may create different outcomes from basically the same input material. An example from Wilks will clarify this:

(2) User. Frank is coming tomorrow, I think

System. Perhaps I should leave (I)

User. Why?

System. Coming from you, that is a warning.

User. Does Frank dislike you?

System. I don't know (II) but you think he does, and that is what is important now.

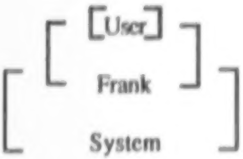
The problem in this example results, argues Wilks, from the fact that beliefs of different types are being run in different environments. The basic issue is that one needs to distinguish between the user's beliefs about Frank's beliefs, the system's beliefs about Frank's beliefs, and Frank's actual beliefs. At points (I) and (II) the system is 'running knowledge about individuals in different environments'.

Wilks uses the following notational approach to represent belief relations.

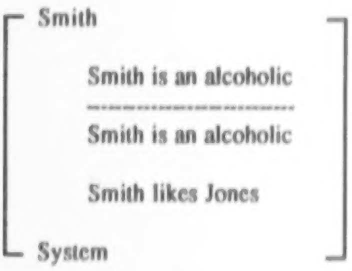
[Frank]

System

This indicates the system's beliefs about Frank. Such structures can be nested as in:


User
Frank
System

This represents the system's beliefs about Frank's beliefs about the user. A further distinction is drawn between A's beliefs about B and A's beliefs about B's beliefs. A line is drawn within diagrammatic representations to indicate this distinction.


Smith
Smith is an alcoholic
Smith is an alcoholic
Smith likes Jones
System

Such a distinction is made because it is possible to believe that Smith is an alcoholic without believing that Smith himself believes this.

Now applying this approach to the case of NT, and considering his utterance as an independent unit, we can construct sample belief environments relative to whether the hearer knows or does not know that NT=CODL. If the hearer knows that NT=CODL then beliefs about the CODL at this point in time will be the same as beliefs about NT; consequently, despite the fact that some extra processing may be required this does not seem relevant to the belief environment itself.

(3) (a) H know NT=CODL

NT

NT=CODL

R(BBC)

NT=CODL

R(BBC)

Criticise (BBC)

Hearer

(b) H not know NT=CODL

CODL

?

CODL = R(BBC)

Criticise (BBC)

Hearer

It is clear from representation (b) that any implications or inferences which might be drawn will not be attached to any specific individual. The symbol ? indicates here that since the identity of CODL is not known, it is difficult for the hearer to have beliefs about the beliefs of this unknown individual (this is, of course, not impossible, but certainly highly implausible). In representation (a) we would be capable of drawing conclusions (which can be extended in terms of the number and type of beliefs we run) which are clearly linked to the identity of NT. But, of course, we already knew this. What I want to suggest, however, is that some hearers who know NT=CODL may actually run beliefs in environment (b) as opposed to environment (a); which would mean, of course, that even though they know NT=CODL, in this context they do not attach conclusions to a specific identity.

Persevering with Beliefs

The problem with my suggestion that speakers who know NT=CODL may nevertheless run beliefs in an environment where he is not specifically identified, is that it seems to be counter to common sense. On the other hand, it would not be possible for speaker/hearers to bring to bear every item of possible relevance to each and every utterance. This is the whole point of Sperber and Wilson's (1986) theory of relevance. Speaker/hearers must somehow work out the relative importance of certain elements of information; this theory, however, simply explains the necessary principles of relevance, it does not deal with the negotiated nature of such relevance, in that any utterance in context may be n ways relevant (perhaps all equally compatible); the hearer's interpretive conclusion is guided by the belief set operating at a particular moment in time. One factor which plays a part in this process is the speaker/hearers' own specific motivations at a particular point within interaction. Speaker/hearer motivation (along with contextual input) helps explain how the referential oddity of the kinds of examples noted by Nunberg (1978; 1979; cf. Brown and Yule, 1983) can be understood. Consider the following case, where a waiter who is going off duty might say: *the ham sandwich is sitting at table 20.* In part, such a phrase will make sense to the hearer only in so far as he/she is motivated to search for a link between the referring phrase and an actual entity (a customer as opposed to an actual sandwich). The link may be clear within an abstract model of discourse which contains a script for waiters and their behaviour, but ultimately it is the hearer's prerogative to interpret the utterance in relation to his own needs at a particiular point in time. Consequently, for a hearer less interested in proving that individual responsibility for attacking the BBC lies with NT, there may be less motivation to run beliefs about the CODL referentially (as NT) as opposed to attributively (as whoever he may be) (see Gibbs, 1987: 582 on some relevant experimental evidence related to selective reference location).

Evidence for this suggestion can be found in one view of the way in which beliefs are organised. Social psychologists have noted in experiments where a subject's beliefs have been manipulated, that such subjects find it difficult to re-adjust their belief system when the 'contrived and inauthentic nature' of the information they had been given is revealed (see Ross and Anderson, 1982; Harman, 1986). It has been suggested that many beliefs, once established, are maintained by a kind of 'habit theory' (Harman, 1986: 37), and that such habits may even be neurologically salient (see Goldman, 1978; cf. Harman, 1986).

Taking up a point made earlier, that a speaker would be expected when talking of himself/herself to make it clear that that is what they are doing (see Boer and Lycan, 1980; 1986); and treating this expectation as a general belief which we would accept unless motivated to reject otherwise, then for those hearers who know NT=CODL, but who are not motivated to pay particular attention to such a fact, the habit of believing that where a speaker talks of himself he will make this clear, may lead them to run beliefs within an environment similar to that constructed by hearers who do not know NT=CODL. Put simply, since NT has not used any explicitly available self referential form he is not referring to himself. This is a kind of default argument, whereby unless the hearer is motivated otherwise, all speaker generic references to self will be treated as attributive rather than referential.

The problem here, of course, is that such an argument seems to contradict the classic Gricean view, that when speakers flout maxims of behaviour they imply information above and beyond the surface interpretation of the utterance itself. But this is of course a speaker intention. If the hearer, for his own purposes, can make sense of the speaker's utterance using the surface form alone, and there is no self-motivating reason to process the utterance any further (hearer intention) then he/she is free to do so. Example cases of conversational implicature in the literature are frequently extreme, in that no further sense can be made of the ongoing discourse without recourse to some implicated information. In the case of the NT utterance, hearers who are aware that NT=CODL can still make sense of what is said without explicitly accessing the fact that NT=CODL, they are free to ignore such facts. We should not assume that because our theory suggests further information can be gleaned by processing implicatures that it is compulsory for hearers to do so. I can find nothing wrong with the following interchange:

(4) A. NT was just trying to worm his way out of the situation

B. No he was just indicating that he was doing his job

C. What do you mean?

A. Well NT is the CODL

C. Of course, that's right, but its not really relevant. Someone has to deal with the BBC.

In (4) speaker (A) has processed the information that NT=CODL and come to a particular conclusion about this; (B) has processed the same information and come to a different conclusions; (C) didn't process the information at all, although he had access to such information, but for (C) the information isn't relevant anyway since he/she sees a general logic to the argument relative to whoever the CODL is.

Taking account of such facts, my argument is not a contradictory of Grice's position but a complementary component. As Johnson-Laird and Garnham (1980) suggested, information is processed relative to both hearer and speaker models of the world. To suggest of any utterance that it carries an implicature indicates only a potentiality for interpretation. Speakers may intend an implicature to be calculated or they may not; hearers may calculate an implicature or they may not. Speaker/hearers perform interpretation in terms of their own interests and motivations, these may coincide for discourse processing, but in many cases this is not a *sine qua non*. Consequently, we should not think of relevance as an optimal informational state jointly agreed by participants (as in Sperber and Wilson, 1986). This may be the ideal, but in real time discourse there are too many intervening variables to guarantee the complete co-ordination of speaker/hearer interests and interpretations.

If my argument is correct it increases the validity of choosing a referring form which, while self referential, could be treated as attributive, particularly in those circumstances where one wishes to deflect personal responsibility. Since one cannot deter the motivated hearer from tagging you with blame, one can at least attempt to offset this fact by leading the general audience to either a non-identification-based conclusion, or an identity-based conclusion with the added, and mitigating, implicature that the speaker is only doing his job.

This view further suggests that one must be careful in extrapolating from theories of relevance to the processing of relevance in the real world. As Johnson-Laird and Garnam (1980) point out, it is possible for the speaker and hearer to operate with different views of the world (which may be adjusted, if necessary, in processing input). Consequently, relevance is itself relative to the contents of speaker/hearer models of the world, and the speaker/hearer's motivation in processing and accessing certain information within such models.

Summary and Conclusion

The argument here has been that, in some circumstances, when a speaker uses certain kinds of definite description to self refer, he/she may be attempting to deter the hearer from attaching any beliefs or associations, connected with whoever or whatever is delimited by the definite description, to the speaker him/herself.

How this is actually achieved is difficult to specify in exact terms. But I have argued here that where the hearer is not motivated to seek a referentially specific entity he/she may simply treat the description as generic or attributive and run any beliefs about the referring description in these terms; with the consequence, of course, that any conclusions or inferences which follow from the belief environment are not bound to any designated individual. Even where the hearer does run beliefs in a referential mode, the extra processing effort may lead to a mitigating implicature (but this is not guaranteed).

The argument is of course theoretical, bounded by one main example, although, as I suggested at the beginning of the paper, the behaviour I was concerned with is one readily recognisable in everyday interaction. It would be useful however, to empirically consider whether the overall distribution of the kinds of example discussed above can be generally found where the speaker wishes to protect himself, or some other individual. Work is underway here (see Wilson in progress; also Maitland and Wilson, 1987) and the initial, and tentative answer, seems to be positive. Further, work on selection within discourse processing (see Brown and Yule, 1983: Ch. 5) does indicate that speakers' interpretations are affected by their own general and idiosyncratic interests. Consequently, the suggestion that belief environments may be limited and constrained by conventional expectations, even under conditions where information for modifying the environment is available, is certainly plausible.

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LEXICAL DENSITY IN INTERVIEW AND CONVERSATION[*]

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1. Introduction

Lexis is a potential indicator text type, in that variation in lexical frequency, lexical complexity and lexical relations can differentiate between types of spoken or written discourse (Ure, 1971; Halliday, 1985; Stubbs, 1986). This paper investigates lexical density (LD) in two varieties of spoken discourse: interview (INT) and conversation (CON), the data being produced by the same subjects. The hypothesis explored is that at least one source of LD variation is personal maturity. However, since inter-individual variation cannot be explained entirely on the basis of this factor, other socio-psychological parameters are clearly relevant.

2. Lexical vs. grammatical items: definitional comments

For Lyons (1986), Robins (1964) and Palmer (1976) lexical items are the major content words, which fall into four grammatical categories: Nouns, Adjectives, Adverbs and Main Verbs. Grammatical items (or function words) serve to express relations between content words, and include: Auxiliary Verbs, Modals, Pronouns, Prepositions, Determiners and Conjunctions.

Sorting words into lexical (L) as opposed to grammatical (G) sets is of course not entirely straightforward. In the so-called phrasal verbs,

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the status of the preposition or particle element is sometimes difficult to determine. For example in:

(1) She made up her face (from Halliday 1966: 153)
(2) She made up her story
(3) They made up and kissed
(4) She made up the hill at speed

the grammatical object in (1) is optional (as in *She made up swiftly*), but not so in (2). The *up* in (1) would therefore appear to be more of an adverbial particle than a preposition, and is therefore directly comparable with the particle status of *up* in (3). In (4), *up* is of course prepositional. The point is that:

(5) They made up

is ambiguous as between meaning (1) and meaning (3). The implication is that in a sensitive analysis, the grammatical status of *up*, can only be resolved by reference to the lexical context. For the purpose of statistical analysis of lexical versus grammatical words, the distinction between the subcategorisation of *make up* has to be ignored, in favour of a cruder classification. In our calculations and for the purposes of this paper, we have followed both Ure (1971) and Stubbs (1986) in considering such phrasal verbs as *make up* as consisting of two words, one lexical word *make* and one grammatical *up*.

3. Preliminaries to Research on LD

Previous research of LD has shown that the concept of 'density' (i.e. ratio of L to G items within a text) can allow texts to be ranked in relation to each other. In very general terms, the ratio of L to G items will show how lexically dense one text is as compared with another.

Lexical density is a property of text, to be calculated in terms of the frequency of L and G items. Information density is a property of processing, for which there is no valid absolute statistic, in that the same text, with its definable LD, will represent different information processing loads for different readers. Space does not permit this point to be expanded. The reader is referred to information processing theories (e.g. Schank, 1975).

Two approaches have been used by researchers to arrive at the G:L ratio in the literature on spoken and written discourse. The first approach is manual, whereby the status of all words in a text is noted by the analyst, after which percentages are worked out (see for example Ure 1971). The second approach is automatic and depends mainly on computer programmes like the one devised by Stubbs (1986), which was designed to run on the London-Lund corpus of spoken English[1]. The manual approach has a greater degree of accuracy since each problem is dealt with by the human linguist in its real context. However, the amount of text processable is presumably limited. Automatic analysis based on tailor-made software, though efficient and reliable to a great extent, is not without problems. In addition to the problem of phrasal verbs mentioned earlier, other types of problem can also arise, some of which have been reported by Stubbs (1986), which no algorithm can resolve. One such problem is that some of the modal verbs such as *can* and *will* can also occur as main verbs, or as nouns in certain contexts. Auxiliary verbs such as *be*, *have* and *do* can also be G or L according to the grammatical contexts in which they are used. Stubbs (1986) solved such problems in his program by building into it a routine to deal with potentially ambiguous words which are categorised according to their context in running text.

There is of course a more general problem in word classification. What one researcher counts as lexical, another will classify as grammatical. Stubbs (1986), for example, lists *be* as lexical or grammatical. Ure, on the other hand, commenting on her (1971) results, counts it as grammatical, even when it has a more lexical function (as in *if you don't be good...*), (personal communication).[2]

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[1] See Svartvik, J. et al. (1982) for a detailed description of this project of spoken English.

Stubbs (1986) provides a useful list of G words, non-G words being, by implication L words. However, the list may not be exhaustive. While, for example, *anything* and *sometimes* are included, *anyone* and *something* are not.

4. Research on LD in types of discourse:

Ure (1971) manually calculated LD in 34 spoken texts and 30 written texts comprising approximately 21,000 words each. The former texts, all except two, have a strong tendency to have an LD of **less than** (40%), whereas the written texts, all except two, have a strong tendency to have an LD of **greater than** (40%). Although these results are suggestive, they are not conclusive, since different subjects produced the spoken and the written data. This is an important source of variation as shown by Beaman (1984) and Farag (1986). The literature on spoken versus written language is considerable and will not be gone into here.

Stubbs (1986), adopting the program mentioned above to analyse six spoken sub-texts of the London-Lund corpus (op. cit.), which represents recordings of highly educated informants, mostly academics, found a significantly higher LD than is reported by Ure. Stubbs' computer calculations show an LD ranging between (44%) and (56%). He relates the difference between his results and Ure's to the different study methods used in the calculations and the nature of the corpora studied. He also mentions the level of respect, which, as we shall see later, is confirmed by our results as well.

Hasan (1988, forthcoming) compares LD in native and non-native speaker speech in five types of formal and informal types of spoken discourse. He reports his formal native-speaker interviews to have an LD of (47.02%) and informal conversation of (42.48%). These results are similar to the results of the present investigation as we shall see below.

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[2] We are very much indebted to J. Ure for her invaluable comments on her 1971 work and for comments on an early version of this paper.

5. The Subjects

16 subjects (6 postgraduates and 10 undergraduates), who were all members of religious, political and/or cultural societies at the Guild of Students, Aston University, were interviewed by a university chaplain, who knew them all on a personal basis or through religious contact. The interview took the form of a review of personal development over the previous 12 months, the chaplain in each case acting as 'elicitor of insights'. Subjects were recorded being interviewed in pairs using a UHER 4000 REPORT recorder with the microphone about 1 metre from participants in a quiet environment. Immediately following the interview, the chaplain withdrew, leaving the two subjects to chat freely. The pairing of the subjects was elective: each pair representing a 'close friend'.

6. Analysis

Calculations of the L:G ratio were first done manually then computationally. In the second method, two simple computer programs[3] were used to identify all L and G items. The final calculations represent an adjustment of the computer programs so as to take account of ambiguous classification. In all ambiguous cases, context was the basis of the decision. The results and statistical tests of significance[4] are presented below.

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[3] The two computer programs employed are 'ALFSORT' and 'FREQSORT' both devised by Professor Frank Knowles at the Department of Modern Languages, Aston University.

[4] 'Wilcoxon's Matched Ranked Pairs Test' was used to test the statistical significance of the results. Details of this test are to be found in Meddis (1975).

Before presenting LD results for INT and CON, two factors must be mentioned which could affect results: repetition and interview input. In order to differentiate between the output of subjects and the output of the interviewer, Table 1 figures include interviewer output and repetitions, while Table 2 figures exclude interviewer output and repetitions. Each pair of subjects is indentfied as A, B, C, etc. A, B and C pairs are postgraduates, pairs D to H being undergraduates.

TABLE 1
Overall Lexical Density in INT and CON

Pairs	LD in INT	LD in CON
A	48.2%	46.9%
B	47.2%	44.4%
C	50.4%	47.4%
D	46.9%	44.3%
E	47.4%	47.6%
F	45.3%	47.3%
G	48.7%	47.6%
H	43.7%	46.3%
Mean	47.2%	46.5%
SD	2.052	1.379

Table 1 above shows a slightly higher mean percentage of L words in the interview situation. However, the difference is not statistically significant (Wilcoxon Signed Rank Text statistic 10.500, p< 0.147). The higher Standard Deviation in the interview data reflects the greater spread of LD values than in conversation.

In order to assess the influence of repetitions and interviewer input, the L percentages were recalculated excluding these. Table 2 presents corrected figures. It shows that, excluding interviewer speech and repetition from the calculations, again LD is somewhat higher in INT than CON but the difference is not statistically significant (Wilcoxon Signed Rank Test statistic 8000, significance level 0.081). Again, we find that the higher Standard Deviation of the interview data reflects the fact that there is a greater spread of LD values. In other words, it is a less internally consistent set of figures than the conversation set.

TABLE 2
Lexical Density in INT and CON
Excluding Repetitions and Interviewer Speech

Pairs	L in INT	L in CON
A	49.8%	47.9%
B	48.5%	45.7%
C	51.1%	47.6%
D	48.7%	44.9%
E	48.2%	47.9%
F	45.7%	47.4%
G	48.2%	47.7%
H	44%	46.6%
Mean	48.025%	46.962%
SD	2.235	1.126

A comparison of mean percentage values for postgraduates as opposed to undergraduates is revealing.

	INT	CON
Postgrad	49.8%	47.0%
Undergrad	46.9%	46.9%

Although the numbers are too small for valid statistical testing, there would appear to be some evidence that:

(1) undergraduates do not differ from postgraduates, in terms of lexical density, in the conversational setting

(2) undergraduates, who are less mature and have received a shorter period of higher education, do not increase LD in the formal

interview, whereas postgraduates do. This possibility requires further work.

One aspect of the results above so far not discussed is that, as Ure (1971) found, the absolute LD value varies from one participant to another, in the same speaking task.

TABLE 3
Lexical Density in Subjects' speech in INT and CON

Participants	LD in INT	LD in CON
HC	48.1%	47.0%
KW	50.4%	47.8%
JH	48.7%	46.8%
BG	48.4%	44.2%
RH	52.0%	46.4%
DD	50.4%	49.1%
RF	50.0%	45.4%
PM	48.0%	41.3%
HH	47.4%	47.3%
KSH	48.7%	48.5%
AM	46.9%	48.3%
CB	44.8%	45.9%
JC	50.6%	47.9%
GM	46.5%	47.5%
AB	44.1%	46.9%
KS	43.8%	46.3%
Mean	48.0%	46.9%
SD	2.235	1.126

The differences between individual speakers are in general as great as the differences between the two speaking tasks. It is also worth pointing out that the direction of difference is not consistent. There are 4 individuals for whom CON has a higher LD than INT (CB, GM, AB, KS); there are 3 individuals who produce the same or virtually the same LD in INT and CON (DD, HK, KSH); and there are 9 individuals for whom there is a clear step up in LD in INT compared with CON (HG, KW, JH, BG, RH, RF, PM, AM, JC). Thus, almost half the speakers manifest a trend which is not in agreement with the trend established by averaging across the whole population. If lexical density is affected by maturity and educational level, further work paying attention to the output of individuals will be required.

7. Conclusion

The general conclusion is that in the present study, which is an attempt to have the same speakers perform different speaking tasks in a controlled situation, lexical density does not differentiate between discourse modes in a global way. Rather, it differentiates between interview and conversation for the postgraduates analysed. Undergraduates, on the other hand, perform comparably, in terms of lexical density, in both the interview and the conversational setting. Since the population examined is a) small and b) not evenly balanced (as between undergraduates and postgraduates), it is premature to conclude that an absolute statistic for the lexical density of undergraduates and postgraduates can be produced. What is interesting, and worth pursuing further, is the differential between the two groups in skill and/or sensitivity at the lexical level. It would appear that postgraduates, who are more mature and have longer exposure to higher education, adjust their lexical density to match some perceived characteristic of the interview situation. Postgraduates may be more able to compete on an equal footing with the interviewer, and this ability may derive in part from a perception that their own status is close to that of the interviewer. Essentially, what is being suggested is an application of 'accommodation theory', which is well known in social psychology, to the lexical level of linguistic control, as an explanation for the rise in lexical density in the interview situation. The interviewer's drop in lexical density can be seen as a conciliatory gesture, metaphorically the opposite of a claim to status; and this in turn facilitates the closure of the status gap of which the postgraduates are able to make use.

8. General discussion

The LD levels of the spoken data analysed in this work are considerably higher than those reported by Ure (1971) for her spoken data and are generally closer to those reported by Stubbs (1986) and Hasan (1988). Even the lowest percentage obtained is higher than the highest in Ure's spoken data where percentages range from 23.9% (assembling Angel Chimes) to 43.2% (radio sports commentary). In Stubbs (1986), the range is from 44% (business telephone conversations) to 56% (radio state funeral commentary).

The question we would like to now ask is: why do different researchers report very different percentages for apparently identical speaking tasks? (Compare Stubbs's 54% for radio cricket commentary with Ure's 43.2% for radio football commentary; or Ure's 'Life' discussion among students (35.2%) with the figure for conversation between students (46.9%) in the present study).

There may be at least eight sources of variation:-

(1) **basis for calculating LD:** i.e. differences in allocating items to lexical as opposed to grammatical classes.

(2) **expected interruption and length of speaking turn:** longer monologic texts predisposing speakers to higher LD (see figures in Stubbs (1986) and Ure (1971) where spoken texts with higher LD are monologues, such as sermons, House of Commons debates, radio commentaries, or lectures).

(3) **function of component units of text.** In the present study, when units with narrative, informative, inquisitive, argumentative or responsive functions are compared, the hierarchy of LD is informative>narrative>inquisitive>negation/hesitation/hedging. The LD (43.9%) of interviewer speech, which is inquisitive, repetitious, full of hedges, and hesitant is lower than the mean LD (47.8%) of interviewee speech.

(4) **self-consciousness/self-monitoring.** Compare Ure's figures for lecture (39.6%) and recorded language laboratory instructions

(40.9%) with the mean 48.0% in interview, 46.9% in conversation, obtained in the present study.

(5) **personal attribute:** maturity, educational level, confidence. Stubbs (1986) comments that the high LD obtained in his study of the London-Lund corpus could have been the product of the high educational level of the speakers. Similarly, Ure (1971) talks of the influence of the previous experience, skill and education on the performance of her subjects.

(6) **group attributes:** age, sex, educational level, etc. In the present study, undergraduates produce lower LD in the interview situation than postgraduates. It should be noted that group attributes may not always be distinguishable from personal attributes.

(7) **planning time.** Both Ure (1971) and Stubbs (1986) mention this as distinguishing between spoken and written production, and it may also contribute to the monitored/unmonitored distinction.

(8) **topic.** Stubbs presents a different LD for state funeral commentary (56%) as opposed to radio cricket commentary (54%). The same 'genre' with different topic and presumably different textual sub-functions can manifest different LD levels.

It is clearly desirable that all eight factors should be controlled in experimental studies of lexical density, although the difficulties of doing so are not underestimated. Ure (1971) for example has two almost directly comparable texts: a spoken text (LD 32/2%) 'How to repot a plant' and a written text (LD 47.1) 'Planting and soil'. It may be difficult to obtain a direct spoken counterpart of a written text; or, indeed, there may be no direct spoken counterpart. (What would be the spoken counterpart of a television news text, which is normally read aloud from a teletext machine?)

There is scope for applying algorithms such as the one developed by Stubbs (1986) to data as wide-ranging as Ure's (1971), but designed in such a way as to ensure that the same subjects produce contrasted text types, on the same topic. Until we know more about the sources of variation in lexical density, explanation of the functions of variation in lexical density will remain tentative.

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REFLECTIONS ON NOMINAL QUANTIFICATION IN THREE ROMANCE VARIETIES: FRENCH, ITALIAN AND GENOESE

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Introductory Remarks

The question of whether the category QP is to be considered part of the repertory of lexical projections on a par with NP, AP, PP and VP (as appears to be the case in Longobardi and Giorgi's seminal study (to appear) of NP structure and Obenauer 1983, 1984) or whether the label QP is really as Bresnan suggests 'merely a temporary convenience' (1973: 277)[1] seems still to be open.

The aim of this short paper cast within the theoretical framework of Government-Binding theory (see Chomsky 1981, 1986a and 1986b) is to re-examine and develop some proposals put forward in Battye (1987) concerning the internal structure of quantified NPs in French such as those shown in (1) below:

(1) i. J'ai lu [$_{NP}$ beaucoup d' articles] récemment
I've read many (of) articles recently

ii. Pierre s'est brouillé avec [$_{NP}$ trop de collègues]
Pierre has argued with too-many (of) colleagues

iii. Marie sait parler [$_{NP}$ plusieurs langues]
Marie knows to-speak several languages

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[1] My thinking on the syntactic status of the lexical items generally classed as QPs has been much influenced by Ewan Klein's unpublished manuscript 'Determiners and the Category Q'. I do not wish to imply however that Klein would endorse my interpretation of his ideas.

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iv. Nous avons discuté avec [$_{NP}$ chaque candidat]
we have discussed with each candidate

In that paper it was proposed that the nominal quantifiers in (1) (e.g. *beaucoup* 'much', *trop* 'too much') were actually NPs themselves generated under the SpecN position whereas the quantifiers shown in (iii) and (iv) are to be classed as APs generated, however, in the same structural position.

Further arguments for this position based on data taken from French, Italian and Genoese[2] will be reviewed in Section 1, where, in particular, the question of the syntactic status of the empty category in SpecN of NPs in direct object position will be examined. Section 2 will then consider how these proposals can be extended to deal with NPs having a quantificatory interpretation contained in A-bar positions such as these shown in the following examples:

(2) i. (Di) libri ne ho tanti (Italian)
of books of-them (I)'ve many

ii. De livres j'en ai tant (French)
of books I-of-them have many

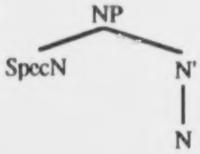
iii. De libri ge n' o tanti (Genoese)
of books 'there' of-them (I)'ve many

---

[2] Genoese is a cover term for the popular dialects spoken in the region of Liguria, whose provincial capital is Genoa. The claims I make here with respect to Genoese are based on informant work carried out in the Summers of 1987 and 1988 with speakers of the varieties of Genoese spoken in Rapallo, Chiavari and Sestri Levante. There exists no orthographical standard for this dialect and therefore the examples given here will use IPA symbols and show conventionally recognized word boundaries.

1.0 Theoretical Assumptions

For our purposes here a simple NP structure like the following will be assumed throughout:



The position referred to by convention as SpecN will be that in which determiners and quantifiers are generated in the Romance languages. N' is considered to be the position under which AP is generated; it will be assumed that this is a potentially recursive node. Finally N is where the head noun is generated. We shall assume that this basic outline will give internal structures for quantified NPs like these below in the three Romance varieties under study here:

(3) i. [$_{NP}$ Plusieurs [$_{N'}$ [$_{N'}$ [$_{AP}$ beaux] [$_{N}$ étudiants]][$_{AP}$ français]]]
many nice students French

ii. [$_{NP}$ Tanti [$_{N'}$ [$_{N'}$ [$_{AP}$ begli] [$_{N}$ studenti]][$_{AP}$ italiani]]]
many nice students Italian

iii. [$_{NP}$ Tanti [$_{N'}$ [$_{N'}$ [$_{AP}$ belli] [$_{N}$ stydenti]][$_{AP}$ italjøn]]]
many nice students Italian

1.1 Two types of Nominal Quantifiers in French

It will be noted that the NP structures shown in (1) and (2) above differ in a crucial way; the NPs shown in (1) contain the preposition de while in those in (2) this preposition is absent. In fact *de* 'of' is obligatorily present in NP structures like those in (1); if it is absent ungrammaticality results:

(4) i. *Beaucoup articles
many articles

ii. *Trop collègues
too-many colleagues

On the other hand if *de* is present in quantified NPs of the type shown in (2) ungrammaticality again results:

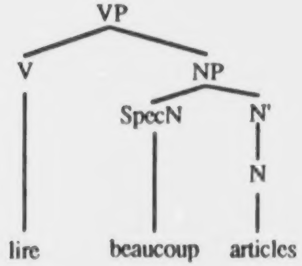
(5) i. *Plusieurs de langues
several of languages

ii. *Chaque de candidat
each of candidate

That the quantified NPs shown in (1) are simple NP structures to be assimilated perhaps to what Selkirk (1977) refers to as pseudo-partitives has been argued in the literature on French (see Milner 1978, Kayne 1981). As has already been noted in order to account for this difference, it has been proposed in Battye (1987) that French quantifiers like *beaucoup* 'much' and *trop* 'too much' should be classed as NPs, whereas quantifiers like *plusieurs* 'several' and *chaque* 'each' are to be considered APs. This difference in categorization has a crucial effect on the distribution of Case (on Case theory see Chomsky 1981) within these quantified NPs.

Chomsky (1986b: 42-4) proposes that the governor of a maximal projection governs not only that maximal projection but also its Spec. Detailed arguments for this proposal and refinements have been given in Longobardi (ms). It must be remembered that abstract Case is assigned under government and that every lexical NP requires an abstract Case if it is not to fall foul of the Case filter, which states that **NP if NP has no Case*. If these two conditions are to be met, then structures like those in (4) are clearly going to be ruled ungrammatical. Let us now consider why this should be the case. If we examine the configuration in (6)

(6)



then it should be clear that the single abstract Case assigned to the direct object position by the verb *lire* 'to read' has two potential recipients: either the whole NP headed by the noun *articles* 'articles' or the quantifier NP *beaucoup* 'many' in the SpecN position. Both are NPs with lexical content and as there is only one abstract Case to be assigned, there is no grammatical output for the configuration as shown in (6) (see (4)i)).

In Battye (1987) it was proposed that structures such as these in (4) and (6) are 'saved' by the operation of an Inflectional Rule (see Borer (1984) for the motivation and theoretical justification of such rules) of the following form:

De Insertion:

$\text{Ø} \dashrightarrow \text{de} \,/[_{NP}\ NP_{+quantifier}\ \_\_\ N']$

De in structures such as those shown in (1) might then be considered to be a Case marker which saves the whole structure from ungrammaticality by providing a second abstract Case which is assigned to the head of the NP; this means that the Case feature provided by the governor of this NP is available for the marking of the quantifier NP in the SpecN position.

With respect to the NP structures shown in (2), where the SpecN contains what we are terming a quantifier AP,[3] there is no need for the insertion of de because the AP does not require a Case marking and so the Case marking provided by the governor of NP will be sufficient to guarantee grammaticality, hence the absence of *de* in the data shown in (2) and the ungrammaticality of the strings in (5).

1.2 The Identification of *vbl* in SpecN

The proposal outlined in the preceding section gains in plausibility if we consider its application to the structures shown in (7) below, usually referred to as examples of *quantification à distance*:

(7) i. J'ai beaucoup lu de livres
I've many read of books

ii. Nous avons peu visité d' amis
we have few visited of friends

iii. Combien crois-tu qu' il a vendu de voitures?
how-many believe-you that he has sold of cars

In order to account for the existence of structures of this kind in French, one might propose that the underlying structure of the object NPs here contain an empty NP variable (*vbl*) in the SpecN position. Fully fleshed out, this proposal would entail that the underlying structure of the VP in (7i) for instance would appear as follows (the proposal that there exists an empty position in these kinds of NP goes back to Kayne, 1975: 30):

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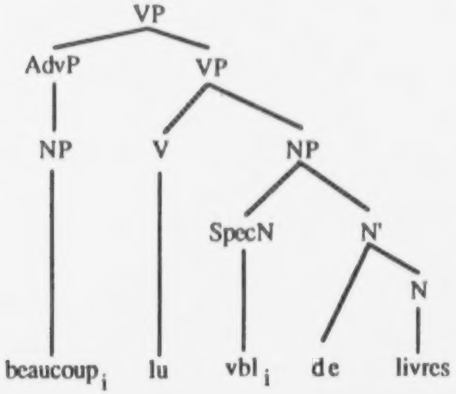
[3] Longobardi (ms: 56-8) presents an interesting alternative to our proposal here. Like us he agrees that *de* in French is inserted as a special Case marker to supplete the Case which has percolated to what is maintained to be QP in Spec N position. He does not, then, follow our line of argument in rejecting the existence of QP; rather two classes of QP are established in French.The first class, corresponding to our AP quantifiers, have overt morphological agreement with the head (incidentally such agreement has a phonological reflex in liaison contexts) and therefore does not absorb Case. The second class of QPs in French (our NP quantifiers) exhibit no overt agreement and for this reason need to absorb the Case assigned by the external governor to the whole NP. I feel that further research may in fact reconcile our two (apparently) opposed positions.

(8)



The conditions necessary for the identification of the *vbl* in SpecN position here can be resumed as follows. Firstly there is an antecedent with which it can be co-indexed (i.e. the NP *beaucoup* 'much' which can also function as an adverbial constituent in French). To be fully identified a *vbl* requires Case (see for instance Chomsky 1981: 175) and if the proposal that SpecN is governed by the external governor of the NP in object position is accepted then once again the abstract Case assigned by the verb here is available for the *vbl*. The fact that the Case assigned by the verb is absorbed by the empty *vbl* in SpecN position means that the Inflectional rule *De Insertion* has to operate here to guarantee grammaticality. One further condition on the identification of a *vbl* is that it be in an A-position (see Chomsky 1981: 185); however here we will follow Obenauer (1983) in somewhat relaxing this condition so that a *vbl* can be identified if it is in an A-position and also if it is part of the

Spec of an A-position.[4] Structures similar to *quantification à distance* configurations with AP quantifiers cannot be found in French because there is no Case available for the identification of a *vbl* in SpecN and also because adjectives cannot be used as adverbials and therefore there would be no antecedent for the identification of the empty category in SpecN.

The analysis that has been sketched out above can also provide insights into the syntax of sentence negation in French and in particular into the distribution of de in such contexts. Consider the following data:

(9) i. Marie n'a pas acheté $[_{NP}$ de pain]
Marie neg-has not bought of bread

ii. Pierre ne veux pas faire $[_{NP}$ d'efforts]
Pierre neg-wants not to-make of-efforts

It could be proposed quite plausibly that the bracketed NPs here in direct object position are to be assigned structural analyses like that shown in (8). Such a position would entail accepting that the lexical item *pas* which appears in negative structures is actually a quantifier of NP status. Such a proposal is indeed quite plausible from a historical point of view since the negative item *pas* derives from the noun *pas*, which in non-negative contexts means 'footstep' (see Harris 1978: 23-9 and references cited there). This point of view leads one to view the *de* in (9) as being the Case marker inserted by *De Insertion* which has already been introduced in this section. In the same way as was seen in the data under (4) the absence of the Case assigner *de* in sentences like (9) produces ungrammaticality:

(10) i. *Marie n'a pas acheté pain
Marie neg-has not bought bread

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[4] Such a condition is quite plausible in the 'Barriers' framework, where Spec-Head agreement plays an important role (see Chomsky 1986b: 24-27 and passim).

ii. *Pierre ne veux pas faire efforts
Pierre neg wants not to-make efforts

1.3 NP quantifiers in SpecN in Genoese?

The answer to the question posed here as the heading of the subsection seems to be a cautious affirmative. Genoese seems to behave like French with respect to the behaviour of certain of its quantifiers, although the repertoire of what are being termed NP quantifiers in Genoese is much more restricted than in French. Firstly let us examine some non-problematical cases in which it seems fairly plausible that one might assume the presence of an NP quantifier in SpecN position. Two NP quantifiers of Genoese are *gwei*[5] 'hardly' and *miga maw* 'quite a bit/lot', both of which behave syntactically in the same way as the class of French NP quantifiers argued for in the last subsection.

(11) i. *o kɔnɔʃy [$_{NP}$ miga maw de dʒente] a sestri
I've known many of people at Sestri

ii. *n'o kɔnɔʃy [$_{NP}$ gwei de dʒente] a sestri
not (I)'ve known hardly of people at Sestri

These examples show, as seen in the French data in (1i-ii), the presence of the Case assigning preposition *de* of Genoese. Were it to be absent then ungrammaticality would be the result:

(12) i. o kɔnɔʃy miga maw dʒente a sestri
I've known many people at Sestri

ii. n' o kɔnɔʃy gwei dʒente a sestri
not (I)'ve known hardly people at Sestri

---

[5] Etymologically the form *gwei* 'hardly' of Genoese is to be related to the NP quantifier *guère* of standard French. It also has an archaic and, today, unproductive equivalent in standard Italian: *guari*.

If we also add that both *gwei* and *miga maw* can be employed as independent adverbials, then it will be predicted that these two NP quantifiers should participate in the Genoese equivalent of the *quantification à distance* configuration. Such a prediction is in fact correct:

(13) i. o miga maw kɔnɔʃy de dʒente a sestri
I've many known of people at Sestri

ii. n'ɔ gwei kɔnɔʃy de dʒente a sestri
neg (I)'ve hardly known of people at Sestri

A more tantalizing case of an NP quantifier is to be found in the behaviour of the nominal quantifier *trɔppu* 'too much' of Genoese. Its interest lies in the fact that two closely related lexical items can be found in the SpecN position of NP: one being *trɔppu* classified as an AP quantifier and therefore having overt morphological agreement, the other being *trɔppu*, an NP quantifier lacking overt morphological agreement and requiring the insertion of *de* before the N' it specifies.[6] Consider the data here:

(14) i. g'o avyw trɔppi prɔblemi
'there'-(I)'ve had too-many problems

ii. g'o avyw trɔppu de prɔblemi
'there'-(I)'ve had too-many of problems

According to the proposed analysis, the absence of *de* in (14) (ii) and its presence in (14i) should produce ungrammaticality in both cases. This prediction proves to be correct as the following data show:

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[6] This double status for *trɔppu* 'too much' is interesting from an historical point of view as it can be interpreted as an example of a lexical item which can be analyzed as either an AP quantifier or an NP quantifier. In fact in Old French some of the lexical items which are today analyzed only as NP quantifiers display this double classification (i.e. *tant*, *trop*).

(15) i. *g'o avyw trɔppi de prɔblemi
'there'-(I)'ve had too-many of problems

ii. *g'o avyw trɔppu prɔblemi
'there'-(I)'ve had too-many problems

trɔppu can be used as an adverbial in Genoese as here

(16) o trɔppu viaʒʒjo.
(I)'ve too-much travelled

In the light of this possibility it is not surprising that with *trɔppu* analysed as an NP quantifier, *quantification à distance* configurations can be found:

(17) g'o trɔppu avyw de prɵblemi
'there' (I)'ve too-many had of problems

On the other hand if *trɔppu* agrees morphologically with its head noun (i.e. meaning that it is employed as an AP quantifier) then the analysis proposed here predicts (correctly!) that no structure equivalent to (17) is possible:

(18) *g'o trɔppi avyw prɵblemi
'there'(I)'ve too-many had problems

The syntax of negation in Genoese presents a problem for the approach to NP quantification adopted here. Consider the following negative sentences:

(19) i. maria a n'a akkattow [$_{NP}$ de pan]
Maria she neg-has bought of bread

ii. piero u nu vø fa: [$_{NP}$ de fadige]
Piero he neg wants to-do of efforts

The analysis proposed for the French data in (9) clearly could account, at least in part, for the structures seen in (19). The obligatory presence of *de* would be the consequence of the 'absorption' of the Case assigned by the verb to the NP in direct object position by a *vbl* in the SpecN of that NP. The major difference here is the absence of a negative adverbial item which might also be analysed as an NP quantifier (i.e. there is nothing corresponding to the French *pas* in (19)). We have, as yet, no specific solution to this problem to offer, but it is not considered to be insuperable and therefore to necessitate a radically different analysis for these structures. An interesting line of inquiry which might yield a solution may be found by investigating the possibility that the negative clitic *ne* (n.b. *n'* in front of a vowel) of Genoese might act as an antecedent for the identification of the *vbl* in SpecN. This possibility might arguably exist in literary styles of French with respect to a very limited class of verbs as shown in (20)

(20) i. Je ne peux manger [$_{NP}$ *vbl* de pain]
I neg can to-eat of bread

ii. Je ne saurais dire [$_{NP}$ *vbl* de mensonges pareils] après
I neg know to-say of lies such after

un tel accident
a such accident

1.4 NP quantifiers in SpecN in Italian?

Standard Italian does not, at first glance, seem to behave in the same way as Genoese or French. In A-positions (on A-bar positions see the next section), the quantifier items which can appear under SpecN all behave like APs. Firstly because they display overt morphological agreement with the head noun:

(21) i. Ho comprato [$_{NP}$ molti$_{(masc., pl.)}$ libri$_{(masc., pl.)}$]
I've bought many books

ii. [$_{NP}$ Poca$_{\text{(fem., sing.)}}$ frutta$_{\text{(fem., sing.)}}$] è stata venduta
little fruit has been bought

Secondly there appears to be nothing similar to Case 'absorption' by items in SpecN, a phenomenon seen in the earlier sections with respect to French and Genoese. Because of this, in A-positions at least, we never find data like that in (22), where the Case marking preposition of Italian (i.e. *di*) has been inserted into the NP:

(22) i. *Ho comprato molti di libri
I've bought many of books

ii. *Poca di frutta è stata venduta
little of fruit has been bought

Particularly noteworthy at this juncture is the absence of the Case marking preposition from negative structures in Italian, thus there is no grammatical string in that language which corresponds to the French data in (9) or the Genoese data in (19):

(23) i. *Maria non ha comprato di pane
Maria not has bought of bread

ii. *Piero non vuole fare di sforzi
Piero not wishes to-make of efforts

Grammatical equivalents to the data in (23) can be found by omitting the preposition *di* as here:

(24) i. Maria non ha comprato pane
Maria not has bought bread

ii. Piero non vuole fare sforzi
Piero not wants to-make efforts

Since the NP in direct object position here has a quantified reading it must be assumed that the SpecN in these cases contains an empty AP quantifier which is absent from the repertory of empty categories in French and Genoese and which does not 'absorb' Case and does not form a context for the insertion of *di*. It can be assumed for our purposes here that this empty AP quantifier in negative structures is identified by coindexation with the negative clitic ***non*** in Italian.

Finally it should also be noted that since Case 'absorption' and *Di Insertion* are not to be found in A-positions in standard Italian, then it is correctly predicted that there will be no equivalent in that language to the *quantification à distance* configuration:

(25) i. *Ho troppo bevuto di caffè
I've too-much drunk of coffee

ii. *Maria ha poco visto di vestiti che le piacciono
Maria has little seen of clothes that to-her please

The data in (25) are ungrammatical despite the fact that *troppo* 'too much' and *poco* 'little' can be found as independent adverbial constituents in Italian:

(26) Ho troppo/poco viaggiato quest'anno.
I've too-much/little travelled this year

To sum up what we have seen in this subsection: it would seem that, at least with respect to the data examined here, standard Italian does not allow NP quantifiers to figure under SpecN and that its repertoire of empty categories contains an empty AP quantifier which is lacking in both French and Genoese. As a consequence of these tentative hypotheses it might be concluded that Italian actually forbids the generation of NP under SpecN and that an equivalent rule to *De Insertion* in French and Genoese is lacking in that language. In actual fact, as will be seen in the next section, where the behaviour of quantified NPs in A-bar positions is examined, both of these claims are too strong.

2.0 A-bar Positions and Empty NP Quantifiers under SpecN

In this section it will be shown that the Left Dislocation data given in (2) (repeated here for convenience)

(2) i. (Di) libri ne ho tanti (Italian)
of books of-them (I)'ve many

ii. De livres j'en ai tant (French)
of books I-of-them have many

iii. De libri ge n' o tanti (Genoese)
of books 'there' of-them (I)'ve many

can be analysed in an elegant way in the context of the proposals that have been made in Section 1 of this paper. It will also be maintained that these structures are important in bringing us to a fuller understanding of the distribution of empty NP quantifiers in the Romance varieties under study here.

2.1 The Clitic Pronouns *EN/NE*

Belletti and Rizzi (1981) have argued that the clitic pronoun ne of Italian, when it pronominalizes part of a quantified NP, should be analyzed as being an N-bar proform. Therefore with a quantified NP structure like *molti libri inglesi* 'many books English', *ne-cliticization* will produce an underlying structural configuration like the following:

(27) i. Hai comprato $[_{NP}$ molti libri inglesi] ?
you've bought many books English

ii. Sì, ne$_i$ ho comprati $[_{NP}$ molti $[_{N'}$ t_i]]
yes of-them I've bought many

where the clitic *ne* is co-indexed with the empty N'-bar position in the object NP. One important consequence of this analysis is that the clitic *ne* does not 'absorb' the Case assigned by the verb to the object position here. This has to be so because the fragment NP which remains after *ne-cliticization* still contains lexical material and therefore this NP will still require abstract Case so as not to fall foul of the Case Filter. Without arguing the point at length it will be assumed that the analysis of the pronoun *ne* given here is also applicable to the pronouns *ne* of Genoese and *en* of French when they are extracted from a quantified NP.

2.2 *De/Di* Insertion in Left Dislocated Structures

Having now mapped out the necessary background on *ne-cliticization*, consider the following left-dislocated configuration in Italian:

(28) [$_{NP}$ [$_{SpecN}$ e] [$_{N'}$ Libri inglesi] ne ho comprati
[$_{NP}$ [$_{SpecN}$ molti] t]
English books of-them I've bought many

In order to understand how this configuration comes to be grammatical, it is crucial that any analysis account for how the left-dislocated NP comes to be Case marked. Cinque (1977) has shown convincingly that a Case relation has to exist between the dislocated position and the position within the sentence in which the dislocated constituent is to be interpreted. For instance an overtly objective Case-marked pronoun cannot be interpreted as coreferential with a subject position and vice versa hence the distribution of asterisks in the following Italian data:

(29) i. Io$_i$/*me$_i$ tutti dicono che e$_i$ sono stato stupido
I/me all say that I've been silly

ii. Me$_i$/*io$_i$ nessuno vuole più vedermi dopo la mia disgrazia
me/I nobody wants more to-see-me after my misfortune

On a descriptive level we may talk in terms of the Case assigned to the sentence internal position being copied onto the dislocated constituent with which it is associated.

Thus in structures such as that shown in (28), it is proposed that the abstract Case assigned to direct object position by the verb *avere* 'to have' will be copied on to the NP in left-dislocated position. In order to understand why, in the context of such an analysis, the structure in (28) is grammatical, it will be proposed that the empty SpecN position is occupied by the empty AP quantifier which it was proposed exists in Italian (see 1.4). It has been claimed that such an empty AP quantifier is absent from the lexical repertoire of Genoese and French, therefore it is correctly predicted that structures equivalent to (28) are absent from these two Romance varieties:

(30) i. *Livres anglais j'en ai tant
books English I-of-them have many

ii. *Libri inglesi ge n'o tanti
books English 'there' of-them-I've many

In fact the equivalent structures to (28) in Genoese and French have to have the following form:

(31) i. De livres anglais j'en ai tant
of books English I of-them have many

ii. De libri inglesi ge n'o tanti
of books English 'there' of-them-I've many

where the operation of the inflectional rule (see 1.0) of *De Insertion* is evident. How can this obligatory presence of Case-marking *de* be accounted for? Since French and Genoese lack an empty AP quantifier then the empty SpecN position in the dislocated NPs in (31) can plausibly be claimed to contain an empty NP *vbl*.[7,8] The presence of this NP *vbl* will, as seen in Section 1, have an effect on the distribution of Case, namely the Case marking copied from the direct object position of the verb in (31) will be 'absorbed' by *vbl* in order for it to be identified as a quantifier expression. A consequence of such a claim is however that the NP headed by *livres/libri* 'books' requires the presence of

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[7] Actually the proposal that a *vbl* may appear in an A-bar position may appear to violate one of the conditions on the identification of a vbl namely that it be in an A-position (see Subsection 1.2). This condition must however be considered to be satisfied if the *vbl* in question can also be associated in some way with an A-position. Association with an A-position will clearly be necessary for the correct identification of certain postverbal vbls in Italian if, as Rizzi (1982: 145-154) proposes, WH-Extraction in that language is from postverbal position. With respect to the dislocated structures dealt with here there would be no problem in associating the NP in A-bar position with an A position.

[8] Obenauer (1984: 180) presents the following contrast as important evidence for the postulation of a syntactic category QP:

(i) ??Combien de filles$_i$ sais-u où inviter t$_i$?
how many of girls know you where to-invite

(ii) *Combien$_i$ sais-tu où inviter t$_i$ de filles?
how many know you where to-invite of girls

According to this analysis the example in (i) is a fairly acceptable extraction from a WH-island because there exists in French an empty NP pronominal (i.e. pro) which the WH-NP *combien de filles* 'how many girls' can A-bar bind. On the other hand there is no similar empty QP category in French (but for a contrary opinion see Longobardi, ms: 58) and therefore the WH-QP *combien* does not A-bar bind the trace in the WH-island and hence the ungrammaticality of this example. The analysis we have proposed here in the text would seem to predict grammaticality for (ii), since we have proposed that pro (i.e. an empty pronominal NP) can appear in Spec N. However Longobardi (ms: 23) has argued that external government of an empty category in Spec N always produces a degree of marginality in configurations involving WH-movement. Adapting this idea to the data in (i) and (ii), it could most plausibly be maintained that the sharply degraded grammaticality of (ii) with respect to (i) results from the combined effect of the WH-island violation and the marginality of external government of an empty category in Spec N.

the Case-marking preposition *de/di*. Since the context for the insertion of this Case-marker is met in these examples such a presence is possible and hence the structures seen in (31).

Considering what has been said, up to present standard Italian, rather unexpectedly, presents grammatical equivalents to structures such as those in (31).

(32) $[_{NP}$ Di libri inglesi] ne ho tanti
of books English of-them-I've many

To all intents and purposes dislocated structures such as these are semantically equivalent to similar structures like that seen in (28). The proposal that this NP might contain an NP *vbl* in SpecN which creates the context for *Di Insertion* would appear to be highly plausible in the light of such data.

Such a proposal requires no modification of what has been said earlier with respect to standard Italian and, in fact, it allows us to clarify the points left unanswered at the end of Subsection 1.4. First of all data like (32) show that Italian does not forbid the generation of NP quantifiers under SpecN, but other conditions in the grammar 'conspire' to restrict this possibility. Secondly it does not seem to be the case that Italian lacks an empty *vbl* which can be identified as an NP quantifier; indeed considering the central importance of empty NP categories in Government-Binding theory, such a gap in the array of possible empty categories would be surprising. Finally the possibility of structures such as (32) shows that Italian does not lack a rule *of Di Insertion.* Therefore the limited distribution of NPs of the form shown in (32) must be due to some other condition.

The rule of *Di Insertion*, it has been proposed, is an inflectional rule in the sense in which this term is used in Borer (1984), where it is suggested that an interesting leading idea would be to investigate whether 'all parametric variation can be reduced to the properties of the inflectional system' (ibid: 4). It seems that the parametric variation identified here between Genoese and French on one hand and standard

Italian on the other might be reduced to a condition on the application of the inflectional rule of *Di Insertion* in Italian. Whereas in Genoese and French this rule can operate freely in all NP positions, it seems to be more restricted in Italian. Tentatively we may propose that this inflectional rule can only apply to NPs in A-bar positions in Italian, although much more comparative research is necessary to establish whether this is the right approach.

3.0 Conclusion

In this paper, an attempt has been made to account for some surface differences between quantified NPs in standard Italian, Genoese and French. The analysis proposed has made specific reference to the distribution of empty NP and AP categories in SpecN. It has also made use of Borer's concept of Inflectional Rules. Although the details of this proposal may not yet be fully worked out, it is felt that the discussion here strongly favours the position of Bresnan (alluded to in the opening paragraph) that the category QP is really only a label of convenience. Indeed it would be difficult to see how the syntactic behaviour examined here could be given an adequate analysis in the context of an approach which viewed QP as an autonomous lexical category.

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ON THE NOTION OF THE IDIOMATIC PREPOSITION: A CASE STUDY FROM ITALIAN[*]

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0.0 The Idiomatic Prepositional Phrase (IPP)

In many languages there are expressions which can be referred to as idiomatic prepositions or complex prepositions, that is, phrases which perform the same functions as simple prepositions. Idiomatic prepositions can be grouped into three main classes according to the types of constituent they contain:

a) adverb + preposition;
b) preposition + preposition;
c) preposition + noun.[1]

Depending on particular theoretical choices these expressions can be considered either from a synthetic point of view, that is, each of them is considered as a whole unit, or from an analytic one, emphasizing their inner constituency. As is well known from studies on the more general problems of idiomatic constructions, for some aspects neither choice is likely to be totally satisfactory because they pose a number of problems

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[*] This article constitutes the English version of a study which appeared as Bottari (1985). The present version contains modifications and refinements with respect to the previous one. I would like to thank Guglielmo Cinque, Luigi Rizzi and participants at the XIII Convegno di Grammatica Generativa held at Trento for helpful comments. Author's correspondence address: Faculty of Foreign Languges and Literature, Università degli Studi di Venezia, Palazzi Foscar e Giustinian dei Vescovi, 30123 Venezia, Italy.

[1] Cf. Regula and Jernej (1965).

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on the descriptive and theoretical level.[2] Instead of taking *a priori* positions on this matter and since there is very little literature on idiomatic prepositions,[3] I shall limit my present examination to a subgroup of phrases of the third class (preposition + noun), aiming to discover some classificatory criteria of theoretical import rather than trying to fix descriptive lines. Furthermore my analysis will concentrate on the Italian language, thus postponing to another occasion more extensive investigation on other languages. Exemplification here will be from Italian even though at many points examples from other languages would meet the purpose of the argumentation.

To begin with, I will adopt a specific label for the restricted class of phrases I am going to analyse. This label, Idiomatic Prepositional Phrases (IPP), at least in the first part of this article, has no particular theoretical status. IPPs have the shape of a PP (Prepositional Phrase) containing an NP whose nominal head governs another PP, as is shown in (1):

(1) $[_{PP_1} P_1 [_{NP_1} N_1 [_{PP_2} P_2 NP_2]]]$

Either the sequence $P_1 - N_1$ or the sequence $P_1 - N_1 - P_2$ (a definite choice makes no difference for the moment) is the IPP. This IPP has the function of assigning a semantic role to NP_2 in ways that happen to be independent or partly independent of the lexical content of N_1. This is shown in the sentences of (2), where the italics marking the IPPs are to be taken as a descriptive convention void of any theoretical or classificatory content: [4]

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[2] Among the many works on the subject the following ones are frequently referred to: Chafe (1968), Gaatone (1981), Fraser (1970), Katz (1973), Ruwet (1983).

[3] As far as I know Gaatone (1976) and Gross (1981) are the only works which treat this problem directly.

[4] Cf. Bottari (1985) for a more detailed analysis.

(2) a. Gianni aveva fatto tutto ciò *a* *vantaggio di* Maria
Gianni had done all that to (the) advantage of Maria

b. Carlo fece un assegno *a* *favore* *di* un suo
Carlo wrote-out a cheque in favour of one (of) his

dipendente
employees

c. Luciano aveva accettato l'offerta *a* *discapito della*
Luciano had accepted the offer to (the) prejudice of-the

carriera del suo collega più affezionato
career of his closest colleague

d. Giovanni si ruppe una gamba *a* *causa* *di* Maria
Giovanni broke his leg on account of Maria

e. Quel giudice fu molto ingiusto *nei* *riguardi del*
that judge had-been very unfair with respect to-the

contrabbandiere
smuggler

f. Carlo odiava tutti *all'* *infuori* *di* Maria
Carlo hated everyone with-the exception of Maria

g. Piero non stimava nessuno *ad* *eccezione* *del* suo
Pietro didn't esteem anybody with (the) exception of his

principale
principal

h. Fellini ebbe dei ripensamenti *in* *merito a* quell'attore
Fellini had some afterthoughts with regard to that actor

i. Gustavo era stato licenziato *all'insaputa* *dei* suoi
Gustavo had been dismissed without the knowledge of his

colleghi
colleagues

j. Il ministro era partito per la Cina *su* *proposta del*
the minister had left for China at the proposal of-the

presidente
president

k. L'uccisione di Cesare *da parte di* Bruto
the killing of Caesar by Brutus

l. Gianluigi mi stava aspettando *di fronte al* palazzo
Gianluigi was waiting for me in front of-the building

m. Avevo una cattiva opinione *al* *riguardo di* quella faccenda
I-had a bad opinion with regard to that matter

0.1 Further Characteristics of IPPs

One of the most significant properties of the IPPs - one that traditionally (though not satisfactorily, as we shall see) is assumed as the prominent classificatory feature for idiomatic prepositions (cf. Regula and Jernej 1965) - is the obligatory presence of a PP after the noun (N_1 in (1)) as the unacceptability of (3) shows:

(3) a. *Carlo partecipò all' impresa commerciale a
Carlo took-part in-the trading enterprise to (the)

vantaggio
advantage

b. *Giovanni si ruppe una gamba a causa
Giovanni broke his leg on account

c. ??Quel giudice fu molto ingiusto nei riguardi
that judge had-been very unfair with regard

A second source of idiosyncrasies is provided by the invariability with respect to number of the N$_1$ in many IPPs:

(4) a. *Carlo fece ciò a vantaggi di Luigi
Carlo did it to (the) advantage of Luigi

b. *Mi dissero parecchie cose a riguardi di Luigi
They-told me many things with regards to Luigi

Another important aspect of these constructions is the fact that the N$_1$ contained in some of them does not occur - or does not occur any more - in free contexts. Among the examples in (2), N$_1$s of this type are *infuori* 'exception', *insaputa* 'lack of knowledge', and, to a certain extent, *discapito* 'prejudice'. Analogously, there are N$_1$s which, even though occurring in free contexts, take on a different and unique meaning when employed in IPPs. In the examples seen in (2) this is the case with *riguardi* 'regards', *riguardo* 'regard', *parte* 'part' and *merito* 'regard'. Parallel observations can be made with regard to the variability of the lexical and categorial selection of an N$_1$ according to whether it occurs in an IPP or in other contexts. Thus, PPs like those in (5), (7), and (9) are acceptable while PPs like those in (6), (8) and (10) are not: [4]

(5) a. Col vantaggio di Pietro su Carlo
with-the advantage of Pietro over Carlo

b. Col suo vantaggio su Carlo
with his advantage over Carlo

(6) a. *A vantaggio di Pietro su Carlo
to (the) advantage of Pietro over Carlo

b. *A suo vantaggio su Carlo
to his advantage over Carlo

(7) Parlò a lungo sui vantaggi dell' iniziativa per Piero
he-spoke at length of-the advantages of-the enterprise for Piero

(8) *Parlò a lungo a vantaggio dell' iniziativa per
he-spoke at length to (the) advantage of-the enterprise for

Piero
Piero

(9) Parlò a lungo sui vantaggi di Piero nell'
he-spoke at length on (the) advantages of Piero in-the

iniziativa
enterprise

(10) *Parlò a lungo a vantaggio di Piero nell'
he-spoke at length to (the) advantage of Piero in-the

iniziativa
enterprise

To these, other aspects could be added, perhaps following the nine descriptive criteria (or idiosyncrasies) proposed in Quirk et. al. (1972). But the sparse observations so far made are sufficient to show that the presence of IPPs in Italian (as well as in other languages) may be held to be uncontroversial. The real difficulties arise when one tries to define exactly what an IPP is.

1.0 Towards a Definition of IPPs: Theoretical Problems

1.1 Possible Approaches

A complete survey of PPs showing idiosyncratic behaviour would give both a list of idiosyncrasies and a list of PPs. The question to be asked at this point is whether there are reasons to consider these PPs to be a special class (i.e. IPPs). Surely there are scarce theoretical reasons to do so since the label 'idiomatic preposition' has no theoretical status. Indeed, the notion of 'idiomatic preposition', unlike other categorial and functional notions, cannot be easily understood simply by considering distributional phenomena.

From a purely 'logical' point of view, the possible definition of an idiomatic preposition has three possible forms: a) the idiomatic preposition is the locus of some specific idiosyncrasies (that is, certain idiosyncrasies but not others are considered the relevant features of idiomaticity; e.g. the absence of a definite article could be included in them while adjectival modification could be excluded, or vice versa); b) the idiomatic preposition is the locus of ALL the idiosyncrasies (that is, given a set of idiosyncrasies distinctly observed in several IPPs, only the IPPs that show all the idiosyncrasies are to be considered idiomatic prepositions); c) it is sufficient to present one idiosyncrasy for a PP to be included among the IPPs.

As will be seen in a moment the first of these definitions, besides being arbitrary in nature since every set of idiosyncrasies will result from a scarcely motivated choice, gives contrasting results according to the types of idiosyncrasy that happen to be chosen. The second one is nothing more than an idealized construct and even supposing that it worked for some IPPs, it would turn out to be of little use since it would overlook important differences among the idiomatic constructions and, most important of all, a whole series of 'gradient' phenomena. 'Gradient phenomena' as we will see in detail below are phenomena which rather than splitting the IPPs into discrete categories (say plain PPs v prepositions), seem to range them along a continuum or a scale of several steps having plain PPs and lexicalized IPPs (= prepositions) at either end and a whole series of IPPs whose status is uncertain in between. As to the third possibility, it may certainly work on a broad descriptive level, but it is of no use for both a finer classification and a theoretical account. To show these points in some detail let us note that the 'classical' definition of an idiomatic preposition (the obligatory presence of an NP argument) would mean that *in merito (a)* 'with regard to', and *al riguardo (di)* 'with regard to' would have to be excluded from the categorial class on the basis of the acceptability of (11) and (12):

(11) Fellini ebbe dei ripensamenti in merito
Fellini had some afterthoughts with regard

(12) Quel giudice fu molto ingiusto al riguardo
that judge had-been very unfair with-the regard

Similar difficulties arise if one tries to establish other specific criteria on which to base a definition of IPPs. The problems seem to surround the abstract notion of idiomaticity. Consider, for instance, a definition of IPPs based on the intuition that the more idiomatic a construction is, the more it resembles the single word with respect to form, behaviour, and content. According to this approach it might be possible to adopt the well known distinctive criterion for composition and derivation employed in morphology and split the IPPs into those that contain nominal heads without independent existence and those containing nominal heads also occurring in free contexts; only the second type of IPP should then belong to the class of idiomatic prepositions. In the terms of this analysis however items such as *a vantaggio (di)* 'to the advantage of', *a favore (di)* 'to the advantage of', *a causa (di)* 'on account of', *ad eccezione (di)* 'with the exception of', *su proposta (di)* 'at the proposal of', and *di fronte (a)* 'in front of' should be excluded from the class of IPPs because the N_1's they contain have a productive independent usage - an unsatisfactory decision since other defining criteria would treat these constructions as idioms.

On a more general level, the preceding criterion reveals itself to be unsatisfactory because, on the one hand, it is not easy to state which words belong to the Italian language and which ones do not; on the other hand, this criterion can only operate in an absolute manner and cannot account for gradiency.

To account for gradiency, other criteria could be sought. These criteria which might be referred to under the general heading of 'crystallization', would reflect a general principle or idea that the more idiomatic an expression (i.e. an IPP) is, the less variable its internal structure. The variations would include modifications of several types, such as adjectival modification, flexional modification, and so on, that is phenomena which undermine the inner cohesion of an expression in some way by assigning lexical autonomy to its constituents.

It goes without saying that the defining content of 'crystallization' is highly circular, but putting aside matters of theoretical import for the moment, 'crystallization' is scarcely adequate even for descriptive purposes. On the one hand, it gives contrasting predictions. For instance, if we consider the examples in (2), a criterion distinguishing idiomatic prepositions from plain PPs on the basis of the absence of determiners for the N[1] would produce unwanted breaks among the class of IPPs: an IPP like *all'infuori (di)* 'to the exception of', for instance, would be rejected by this criterion in spite of the fact that other criteria of the same type would class it among the idiomatic prepositions. Similarly, a criterion based on the absence of adjectival modification would overlook important differences related to the kind of modifier and the particular constructions in which it occurs, as (13) and (14) show:

(13) a. Mi licenziai a tutto vantaggio di Carlo
I resigned to (the) full advantage of Carlo

b. *Mi licenziai con tutto vantaggio per Carlo
I resigned with (the) full advantage for Carlo

(14) a. *Mi licenziai a grande vantaggio di Carlo
inaspettato
apprezzabile
....

I resigned to (the) great advantage of Carlo
unexpected
appreciable
....

b. Mi licenziai con grande vantaggio per Carlo
inaspettato
apprezzabile
....

I resigned with (the) great advantage for Carlo
unexpected
appreciable
....

The mirror-image situation shown by these examples seems to indicate that there are stronger restrictions on the types of adjectives that may enter into a construction with the IPP *a vantaggio (di)* 'to (the) advantage of' than on the types of adjectives that may enter into constructions with the IPP *con vantaggio (per)* 'with (the) advantage for'. Of course, further investigation would have to explain this behaviour, but, for what concerns us here it only has to be observed that any explanation whatever has to be based on both a theoretical account of the behaviour of the adjectives involved and the acknowledgement of a radical difference between two IPPs.

Clearly there could be certain criteria of 'crystallization' that might produce unquestionable results as regards the discovery of idiosyncratic PPs (cf. point (c) above), but these observations would be too general to be used as classificatory tools or - what is worse - as theoretical tools: they would, in fact, provide no hint to possible differentiations among the broad class of IPPs that could be individuated. Criteria of this kind, for instance, are the rigorous invariability of the N_1 or what we might term the 'pronominalized N' criterion, shown in the following examples:

(15) a. *Carlo era stato molto scortese nei riguardi$_i$ di Mario,
Carlo had been very rude with respect(s) to Mario,

Lucia lo era stata in quelli$_i$ di Rodolfo
Lucia (it) had been with those of Rodolfo

b. *Erano partiti tutti all' infuori$_i$ di Carlo mentre
Everybody had-left with-the exception of Carlo while

a quello$_i$ di Luigi nessuno era arrivato sano
with that of Luigi nobody had arrived safely

(Notice, incidentally, that this criterion is only valid for PPs whose nominal component is modified by a definite article, as required by the properties of the demonstrative pronouns).

The preceding observations should suffice to show that classificatory criteria based on phenomena characterizable in distributive and superficial ways not only give results which are scarcely interesting if not contradictory - an indication of descriptive inadequacy - but they do not even suggest the direction research should take for a sufficiently coherent account of the variety of the behaviour of the IPPs.

1.2 The Theoretical Approach

In contrast to descriptive approaches, a theoretical account[5] of the IPPs will reject general labels such as 'idiomatic preposition', and start directly with an examination of the particular idiosyncrasies characterizing these constructions trying to trace them back to general principles governing the whole grammar, that is, principles that can be thought of as part of a substantive representation of the actual linguistic knowledge of the speaker. This means that the idiosyncrasies of IPPs have to be considered in conjunction with other empirical phenomena and in particular those phenomena that have received satisfactory explanations in some theoretical model. In the remaining part of this survey I will try to provide an outline of what this strategy might be like by taking into account possible characterizations of the categorial nature of a group of IPPs. The properties of anaphoric and pronominal reference as they are dealt with in the Government and Binding version of Generative Grammar (Chomsky 1981) will constitute the main tool for the investigation. The characterization stemming from the anomalous behaviour of these IPPs certainly will not exhaust all the possibilities of analysis - at the logico-semantic level, indeed, there seem to be many lines of research to be followed, as, for instance, the interesting contrasts between (13) and (14) clearly show. In this sense, what follows is simply meant as an illustrative attempt rather than a definitive account, something which justifies the concision of some of the argumentation. However it is considered that the general line of argumentation adopted here is fruitful and when fully worked out it will perhaps yield important theoretical insights.

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[5] Of course, the choice between a theoretical approach and a descriptive one depends on more general and deep aims, such as those represented in Chomsky's recent dichotomy between 'Internal-language' and 'External-language' (Chomsky 1986a), which Ambrosini, following the Saussurian idea of *langage*, has extended to a broader notion of "theoretical linguistics" vs other branches of of linguistic studies (Ambrosini 1987), but it is interesting to note that in the case of IPPs (and, perhaps, in the case of idiomatic constructions in general) a mere descriptivism turns out to be defective.

1.2.1 Introduction to Binding theory and the Binding theory test

The distinctive character of the Binding theory consists in treating semantic phenomena in terms of syntactic configurations - an approach which turns out to be particularly useful for the definition of the categorial status of the IPPs. The semantic phenomena Binding theory deals with are the possible references that different types of NP may take, with particular regard to NPs which do not denote objects or entities in a direct way but which refer to other NPs in the linguistic string or to other elements in the pragmatic context. The syntactic configurations pertaining to Binding theory - that is, the 'Binding domains' - are specific maximal projections (NP and S, as we will see) which bound the possibilities of reference for certain types of NPs that they contain.

Binding theory, then, consists of a classification of the NPs in terms of the different properties they show as regards their possible references as well as consisting of a definition of the forms and the restrictions on the assignment of reference in terms of binding domains. Thus, following the synthesis of Binding theory put forward in Radford (1981), let us assume a classification of NPs into anaphors, pronominals, and lexical NPs (or R-expressions in the terms of Chomsky 1981). An anaphor is an NP that cannot have independent reference and which takes its reference from some other expression in the sentence: its antecedent. Typical English anaphors are the reflexive *himself, herself*, etc., and the reciprocal *each other*. The pronominals (personal pronouns *he, she* etc., possessive *his, her*, etc.), like the anaphors, have no independent reference but, unlike the anaphors, either get their reference from some NP in the sentence or get it in the pragmatic context as is shown by the different indices of *he* in (16) (where identical indices mean identical reference):

(16) John$_i$ thinks he$_{i/j}$ is clever

Finally, the lexical NPs or R-expressions are NPs with independent reference, that is, NPs that, like *John* in (16) need no (syntactic or pragmatic) antecedent.

The forms of the assignment of references (or 'referential indices') are expressed in the three conditions of the Binding theory, which also constitute a finer definition for the corresponding types of NPs:

(17) Binding theory

A. An anaphor is bound in its governing category
B. A pronominal is free in its governing category
C. An R-expression is free

To understand (17) a formal apparatus is necessary that I draw from Radford (1981) (to which I refer the reader for further details):

(18) a. X is *bound* if X is an argument coindexed with an argument c-commanding it; if X is not bound then it is free

b. An *argument* is an NP position in S or NP (subject, direct object, indirect object, etc.)

c. X *c-commands* Y if the first branching node dominating X dominates Y and neither X dominates Y nor Y dominates X

d. X is the *governing category* for Y if X is the least NP or the least S containing a governor for Y

e. X *governs* Y if X is the least potential governor (= V, P, N, A, and TENSE) c-commanding Y and there are no S-bar or NP boundaries between X and Y

Consider then the following sentences:

(19) a. John$_i$ hurt himself$_i$

b. John$_i$ hurt her$_j$

c. John$_i$ hurt Fred$_j$

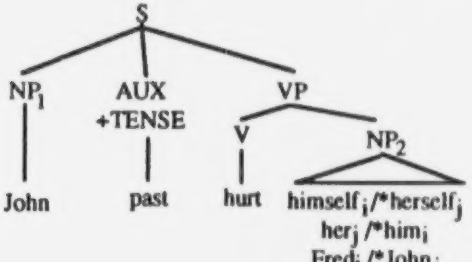
(20) a. *John$_i$ hurt herself$_j$

b. *John$_i$ hurt him$_i$

c. *John$_i$ hurt John$_i$

These sentences have the (simplified) structure (21), which explicitly shows the c-command and government relations:

(21)



S is the governing category of NP$_2$; NP$_1$ can be a possible binder for NP$_2$ since it c-commands NP$_2$. The predictions of the Binding theory (17), then, are entirely met by the examples in (19) and (20). Of course the Binding theory does not tell us how pronominals get their reference in cases like (19b) and others of the same type; or how it is possible that in a sequence like *Napoleon was defeated at Waterloo; however the general did not give up at all*, *Napoleon* and *the general* are interpreted as referring to the same person. Indeed, as regards pronominals and R-expressions at least, the theory simply predicts which references are impossible in which contexts. What is particularly interesting, however, is the fact that the Binding theory (17) establishes a complementary distribution of anaphors and pronominals as is well shown in the contrasts (19a/b) and (20a/b). Now, as far as our survey of IPPs is concerned, it is worth noting that, if it is correct to assume that the binding domains are NP and S and not, for instance, PP, as (22) and (23) clearly show

(22) a. John$_i$ did it [$_{PP}$ for himself$_i$]

b. *John$_i$ did it [$_{PP}$ for him$_i$]

(23) a. *John$_i$ told Mary that [$_S$ Katy did it [$_S$ for himself$_i$]

b. John$_i$ told Mary that [$_S$ Katy did it [$_S$ for him$_i$]

it is correct to infer that, whenever an anaphor and a pronominal show complementary distribution with respect to an identical antecedent there is an NP or S boundary between them. This prediction also obtains in the revised version of the Binding theory that I shall assume in the present work (see below), at least in so far as the unacceptability of a certain index for a pronominal predicts the presence of an S or NP boundary between the pronominal and its antecedents.

1.2.2 The Binding theory test over a group of IPPs

The IPPs I am going to analyse have the form of a PP headed by *in* 'in' or *a* 'to' and containing a nominal head which governs a PP that is always headed by *di* 'of'; they include forms like *a vantaggio (di)* 'to the advantage of', *a favore (di)* 'to the advantage of', *in favore (di)* 'to the advantage of', *al posto (di)* 'in the place of', *a discapito (di)* 'to the prejudice of', *a danni (di)* 'to the prejudice of' etc.[6] All these IPPs manifest identical behaviour with respect to anaphoric elements (*se stesso* 'himself', *se stessa*, 'herself', etc.; *proprio* 'his own', *propria* 'her own', etc.) and pronominal elements (*lui* 'him', *lei* 'her' etc; *suo* 'his', *sua* 'her'). Thus we can assume the following paradigm as valid for all of them: [7]

(24) a. Gianni$_i$ aveva fatto tutto ciò *a vantaggio di se stesso*$_i$
Gianni had done all that to (the) advantage of himself

b. *Gianni$_i$ aveva fatto tutto ciò *a vantaggio di lui*$_i$
Gianni had done all that to (the) advantage of him

c. Giannii aveva fatto tutto ciò *a vantaggio di lei*
Gianni had done all that to (the) advantage of her

d. Gianni$_i$ aveva fatto tutto ciò *a suo*$_i$ *vantaggio*
Gianni had done all that to his advantage

e. Gianni$_i$ aveva fatto tutto ciò *a vantaggio suo*$_i$
Gianni had done all that to (the) advantage his

---

[6] Many types of IPPs are then excluded from the analysis, as, for instance, those having a locative meaning (*di fronte (a)* 'in front of', *in capo (a)* 'at the head of', etc.) or IPPs like *alla maniera (di)* 'in the manner of', *a differenza (di)* 'differently from' to which the Binding theory test is inapplicable. This empirical restriction, of course, cannot affect the results of the present article, whose purposes are not descriptive but methodological.

[7] As to the judgements on the examples I will propose from now on, there are significant variations among native speakers of Italian. To overcome this difficulty I tested the paradigm on a certain number of informants: the variations I registered never affected sentences of the type (30a-c) (which I tested for the full group of IPPs), a fact that will appear to be crucial in the argumentation to follow.

f. Gianni aveva fatto tutto ciò *a proprio vantaggio*
Gianni had done all that to his own advantage

g. Gianni aveva fatto tutto ciò *a vantaggio proprio*
Gianni had done all that to advantage his own

The paradigm (24) can be split into two subgroups of sentences according to the categorial labelling (25):

(25) ...$[_{PP}$ a $[_{NP}$ vantaggio $[_{PP}$ di NP_i]]]

which represents the underlined part of (24a-c), and the categorial labelling (26):

(26) ...$[_{PP}$ a $[_{NP}$ (suo/proprio) vantaggio (suo/proprio)]]

which represents the underlined part of (24d-g). These categorial representations will be modified or will appear to be open to objection at the end of the discussion, but, for a preliminary analysis, they stand as the most obvious starting point.

Let us then proceed by assuming Chomsky's (1986a) version of the Binding theory which represents a modification of the theory I have illustrated in 1.2.1, even though it keeps the central ideas of the Binding theory intact. Contrary to previous versions of the theory which were only able to account for simple alternations between pronominal elements and anaphors,[8] Chomsky's proposal can easily deal with cases like (27), where a pronominal element and an anaphor bearing the same index occur in an identical context:

(27) a. The boys$_i$ saw $[_{NP}$ their$_i$ pictures]

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[8] The literature on the subject amounts to a few titles. Cf. Belletti (1978), Cinque (1981), Giorgi (1985), and Giorgi and Longobardi (in press), for further references also.

b. The boys$_i$ saw [$_{NP}$ each other's$_i$ pictures]

In order to overcome the difficulties that a version of the Binding theory like that in (17) encounters in sentences like (27) (namely, the fact that only (a) satisfies the requirements of (17) while (b) shows the acceptability of an anaphor bound from outside its governing category) Chomsky disjoins the complementary distribution of anaphors and pronominals from the Binding theory proper and posits separate requirements for any one of the three types of NP involved in the theory; in particular, he tries to formalize the property that anaphors show of taking antecedents outside the minimal NP or S dominating them. The formal apparatus Chomsky elaborates to account for the observed phenomena reaches a high degree of complexity. The reasons for this are to be sought not only in the requisites arising from empirical observations but also - and perhaps, mainly - in some new general issues stemming from the reflections on the theoretical status of the generative model. The new format of the theory consists of the definitions (28) and (29) plus the definition of 'Complete Functional Complex' which I quote directly from Chomsky (1986a).

Assuming an indexing I, an element α, a local domain β, and a lexical category γ governing α, Chomsky advances the following formulae:

(28) I is Binding Theory-compatible with (α,β) if

A. α is an anaphor and is bound in β under I
B. α is a pronominal and in free in β under I
C. α is an R-expression and is free in β under I

(29) **Licensing Condition**

For some α such that (i) or (ii), I is Binding Theory compatible with (α, β):

> i) α is an R-expression ...
>
> ii) α is an anaphor or a pronominal and β is the least Complete Functional Complex (CFC) containing γ for which there is an indexing J, Binding Theory compatible with (α, β)

The concept of Complete Functional Complex is defined in terms of the Government theory and the Extended Projection Principle:

> A governing category [for α] is a maximal projection containing both a subject and a lexical category governing α (hence containing α). A governing category is a 'complete functional complex' in the sense that all grammatical functions compatible with its head are realized in it - the complements necessarily, by the projection principle, and the subject, which is optional unless required to license a predicate, by definition (Chomsky 1986a: 169)

In informal terms the sense of these formulae is that the relevant binding domain for an anaphor or a pronominal is a sentential domain, which means either NP or S, and more importantly, that this domain need not be the same for both categories. The binding domain for pronominals is always a very restricted one because it serves the purpose of excluding unwanted coindexations. The binding domain for the anaphors, even if it remains constrained in various ways, happens to be larger than the binding domain for the pronominal elements since the anaphors can only be coindexed with an element in the sentence, in other words, if an anaphor finds no coreferent in the minimal NP or S dominating it, the coreferent has to be searched for outside those categories (with due limitations). Thus, contrary to previous versions of the theory, (27b) is accounted for by simply assuming the whole S as the minimal binding domain for the anaphor *each other* just because NP, a possible binding domain too, contains no indexing, hence no indexing which is Binding Theory-compatible with *each other*. In (27a), instead, NP functions as the minimal binding domain for the pronominal *their* for exactly the same reason: it contains no indexing for *their*, which, thus, is free in terms of an indexing I as required by condition (B) of

(28). In connection with the present article and what I have observed in the final part of 1.2.1, it is important to stress the fact that in this new version the predictive power of the Binding theory as to the categorial environment has been modified only with respect to anaphors and not with respect to pronominals: an unacceptable indexing for a pronominal still implies that the pronominal is bound in the minimal NP or S dominating it.

Let us now turn to the examples in (24), considering (a), (b), and (c) first. The version of the Binding Theory we are assuming predicts the acceptability of all the three sentences since the pronominal elements and the anaphors occur in a context which is analogous to the one in (27). The ungrammaticality of (24b), then, remains problematic for the Binding Theory and mysterious too, considering that this theory seems to be perfectly adequate in dealing with analogous empirical contexts such as (30):

(30)	Marco$_i$	ci fece	avere	[$_{NP}$ quella	descrizione	di lui$_i$]
	Marco	let us	have	that	description	of him

The contrast between (24b) and (30) suggests that the solution should not be sought in possible modifications of the Binding Theory, nor is it to be sought in the binding phenomena generally, but somewhere else. What is still more amazing in this respect is the full acceptability of (24d and e). These sentences, for the purposes of the Binding Theory, show a categorial context which is identical to the one in (24b) (that is, the NP functions as the minimal binding domain for the pronominal elements *lui* 'him' and *suo* 'his'); this time, however, the indexing is acceptable, just as the Binding Theory predicts. Now, any hypothetical modification of the Binding Theory to account for the unacceptability of (24b) - namely, an extension of the binding domain to the whole S - not only would have to be justified on general grounds, but would also have to explain this contrast. Such an attempt would be contradictory since it would result in a theory predicting the possibility and impossibility of occurrence of the same element in the same type of context.

If these conclusions are correct, then, we can rightly say that the solution to the problems raised by the unacceptability of (24b) are to be sought outside the Binding Theory or outside the binding phenomena generally. The possibilities that come to mind are of two types: a) reinterpreting the categorial representation of (24b) so as to meet the requirements of Binding Theory; b) looking for other grammatical restrictions that disallow the pronominal element in (24b) independently of the predictions of the Binding Theory. The first solution could encounter difficulties both in justifying any decision on categorial interpretation for (24b) that would differ from the most intuitive one (namely, the one shown in (25)) and in justifying its inapplicability to other sentences of the paradigm (24), that is, (24d, e) and, perhaps, (24a, f, g). A solution of the second type would avoid all these difficulties since no categorial rearrangement would be implied in it, a simple justification of the impossibility of having a pronoun in a structure like (24b) being sufficient. The sole problem would be the acceptability of (24c) which contains a pronoun in a context similar to that of (24b); however, if this difficulty were overcome, it would turn out that IPPs like *a vantaggio (di)* 'to (the) advantage of' are in no way different from plain PPs – a crucial conclusion which would strongly weaken the idea that there should be PPs having a 'special' idiomatic status, that is, idiomatic PPs.

1.3 Possible Solutions

A solution of the second type alluded to in the previous subsection seems to be readily at hand. In Italian, as in other languages, *di* + pronominal sequences, and *di* + anaphor sequences or, more generally, sequences of the type *di* + non-Referential expression are not allowed to occur freely where they can be substituted by an appropriate possessive element (which still bears pronominal or anaphoric features). This restriction, which I shall call the Possessive Generalization for lack of a more suitable theoretical label,[8] is responsible for the ungrammaticality of the sentences in (31) and (32), which, of course, will become grammatical as is shown in (33) and (34), when the *di* + pronominal/anaphor sequence is substituted with the appropriate possessive element, *suo* 'his' in (31) and *proprio/propria* 'his/her own' in (32),[9] that is, when the Possessive Generalization has been applied:

(31) a. */?? Mauro$_i$ chiamò il cane di lui$_{i/j}$
Mauro$_i$ called the dog of him$_{i/j}$

b. */? L' ambasciatore$_i$ confermò la partenza di lui$_{i/j}$
the ambassador$_i$ confirmed the leaving of him$_{i/j}$

c. */?? Carlo$_i$ pagò la telefonata di lui$_{i/j}$
Carlo$_i$ paid (for) the 'phone call of him$_{i/j}$

(32) a. *Mauro chiamò il cane di se stesso
Mauro called the dog of himself

b. *L' ambasciatore confermò la partenza di se stesso
the ambassador confirmed the leaving of himself

c. *Carlo pagò la telefonata di se stesso
Carlo paid (for) the 'phone call of himself

(33) a. Mauro chiamò il suo cane
Mauro called (the) his dog
b. L' ambasciatore confermò la sua partenza
the ambassador confirmed (the) his leaving

c. Carlo pagò la sua telefonata
Carlo paid (the) his 'phone call

(34) a. Mauro chiamò il proprio cane
Mauro called (the) his own dog

---

[9] The judgements of informants are not uniform for the examples in (31), especially for (31b) (cf. Bottari 1985 for explanations). These variations, however, are quite independent from the indexes the pronominal can bear (thus there is no relationship between the alternating symbols in front of the examples and the alternating indexes).

b. L'ambasciatore confermò la propria partenza
the ambassador confirmed (the) his own leaving

c. Carlo pagò la propria telefonata
Carlo paid (for) (the) his own 'phone call

On the basis of these facts, then, the ungrammaticality of (24b) could be accounted for without resorting to binding phenomenology. This solution, however, cannot be considered a viable one since it raises too many problems and some overt contradictions. Let us consider them in turn.

As (31) shows, the Possessive Generalization operates independently of the index of the pronominal, but this fact openly contrasts with the acceptability of (24c) (which I repeat) and, even more crucially, with the acceptability of (35), identical to (24b) in all but the index of the pronominal:

(24)c. Gianni aveva fatto tutto ciò *a vantaggio di lei*
Gianni had done all that to (the) advantage of her

(35) Gianni_i aveva fatto tutto ciò *a vantaggio di* $\textit{lui}_j$
Gianni_i had done all that to (the) advantage of him_j

Another crucial fact is represented by the acceptability of (24a), repeated here

(24) a. Gianni_i aveva fatto tutto ciò *a vantaggio di se* $\textit{stesso}_i$
Gianni_i had done all that to (the) advantage of himself_i

which now becomes problematical given the unacceptability of the sentences in (32).

The Possessive Generalization does not, then, seem to serve the purpose of justifying the strange behaviour of *a vantaggio (di)* 'to (the) advantage of' in (24). However, before abandoning this solution, let us consider some possible ways out of the difficulties we have just observed: their ultimate ineffectiveness will simply reinforce the conclusions we have come to. To put it briefly, there are conditions or contexts which block the application of the Possessive Generalization: if it turned out that there are reasons to suspend the application of the Possessive Generalization in (24a, c) and (35) but not in (24b), the whole account could be kept as valid. As Cinque (1980, 1981) has demonstrated in detail, the Possessive Generalization applies only optionally or does not apply at all depending on the type of nominal head, when the *di* + non-R-expression sequence functions as the 'object' of the NP.[10] The NPs in (36) are indeed acceptable under this interpretation:

(36) a. Il desiderio di lui
the desire of him

b. La descrizione di lui
the description of him

c. Il ritratto di lui
the portrait of him

Nouns of the type of *vantaggio* 'advantage', however, do not seem to select an 'object' argument, as the unacceptability of (37) shows

(37) */?? Discutemmo i vantaggi di lui
we-discussed the advantages of him

(see similar examples with anaphors also below).

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[10] To be more precise, it should be said that a *di* + non-R-expression sequence can freely occur under the N-bar level of an X-bar projection of N. As recent studies have shown, it is inappropriate to call this position the 'object' position of NP since there are intransitive nouns like *partenza* 'leaving/departure' which head an NP whose seeming subject is to be interpreted as generated in this position. These nouns parallel the corresponding verbs which, in the literature, after Burzio (1981), are referred to as 'ergative' verbs. On what we may call 'ergative nominals' see the pioneering article of Cinque (1981) and recent research in Giorgi (1985), Giorgi and Longobardi (in press), and Bottari (1985).

Another possibility of suspending the application of the Possessive Generalization has been pointed out by Belletti (1978). Belletti's detailed study shows that when a *di* + pronominal sequence[10] can disambiguate a sentence by means of the overt gender features contained in the pronoun (and absent in the corresponding possessive) the Possessive Generalization does not apply even if the pronominal element does not cover an 'object' function:

(38) a. Mauro chiamò il cane di lui non quello di lei
Mauro called the dog of him not the one of her

b. *Mauro chiamò il suo cane non il suo
Mauro called his dog not hers

Similarly, as has been observed by Giorgi (1985), the Possessive Generalization does not apply when the pronominal element (or the anaphor) is followed by an appositive expression:

(39) a. Gianni detesta quel giudice di lui piccolo che lo fece
Gianni loathes that judge of him when a child who had him

mettere in riformatorio
put in (a) reformatory

b. Maria detesta gli ammiratori di lei in costume da bagno
Maria loathes the admirers of her in bathing suits

Again nouns like *vantaggio* 'advantage' do not conform to these requirements or possibilities when they occur in IPPs, or, at least, they do not when the pronominal element is coindexed with the subject of the sentence:

(40) a. Gianni_i aveva fatto tutto ciò a vantaggio di $\text{lui}_{*i/j}$ e
Gianni_i had done all that to (the) advantage of $\text{him}_{*i/j}$ and

dei suoi familiari
of his relatives

b. Gianni$_i$ aveva fatto tutto ciò a vantaggio di lui$_{*i/j}$ non
Gianni$_i$ had done all that to (the) advantage of him$_{*i/j}$ not

di lei
of her

c. Saragat$_i$ aveva fatto tutto ciò a vantaggio di lui$_{*i/j}$,
Saragat$_i$ had done all that to (the) advantage of him$_{*i/j}$,

futuro presidente della Reppublica
future president of the Republic

Finally, anaphors seem to escape less easily from the effects of the Possessive Generalization than do pronominal elements. In short, a *di* + anaphor sequence cannot be accepted when it functions as 'subject' of NP, even in contexts where the relaxation of the Possessive Generalization might be possible: [11]

(41) a. *Gianni pagò l' appartamento di se stesso non quello di
Gianni paid (for) the flat of himself not that of

sua madre
his mother

b. *Greta Garbo andava fiera dell' interpretazione di se stessa in
Greta Garbo was proud of the interpretation of herself in

costume da bagno[12]
(a) bathing suit

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[11] According to the observations in footnote 10, it should be said that inside an NP a *di* + anaphor sequence can only occupy a position dominated by an N-bar node. For a more detailed account of this restriction cf. Bottari (1985); for an explanation based on further observations on the nature of the anaphors see Graffi (1987).

[12] Of course, (45)b is acceptable under the reading *Greta Garbo interprets Greta Garbo in a bathing suit.*

The problem, as observed in footnote 11, is too complex to be fully illustrated here, but, even this brief survey will suffice to show that a sentence like (24a) cannot be accounted for following this line of argument. Indeed, nouns like *vantaggio* 'advantage' or others occurring in the IPPs we are considering, when employed in unmarked NPs behave exactly like *telefonata* 'phone call' (see 32c) and *interpretazione* 'interpretation' (see 41b) in so far as they do not accept a *di* + anaphor sequence, which, presumably (or consequently) is to be interpreted as the 'subject' of the NP: [13]

(42) a. *Mario ci aveva parlato dei
Mario had spoken to us of the

vantaggi/danni/favori/meriti di se stesso
advantages/damages/favours/merits of himself

b. *Carlo parlò dei vantaggi di se stesso non di quelli di
Carlo spoke of the advantages of himself not of those of

Giorgio
George

c. *Leone apprezzava i vantaggi di se stesso Presidente
Leone appreciated the advantages of himself as President

della Repubblica
of the Republic

However, the full acceptability of (24a), which does not exemplify either a context nor one of the conditions blocking the application of the Possessive Generalization, again stands as a strong contrast to the normal behaviour of the nominals and constitutes an additional problem with respect to those outlined at the beginning of the discussion.

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[13] Or, as generated directly under N-double-bar.

2.0 Towards an Explanation

From the evidence just reviewed it can be concluded that the Possessive Generalization and connected phenomena do not seem to offer an explanation for the ungrammaticality of (24b), which, as a matter of fact, must ultimately be attributed to binding factors. Furthermore, the acceptability of (24a) also has to be justified in some way. More generally, the failure of this attempt forces us to resort to the other one we considered above, namely, to reinterpret the categorial structure of the sentences so as to meet the Binding Theory requirements. As already pointed out this section is not an easy one to work out because of the numerous problems it raises. Indeed, sentences (24d-g) behave perfectly with respect to both the Binding Theory and the Possessive Generalization as if they were neatly distinct from (24a-c), a fact that renders the task of finding the exact terms of the solution even harder. However, it is reasonable to say that the exceptional behaviour of sentences (24a-c) calls for an equally exceptional solution - and one that may also apply optionally, thus not applying to (24d-g) - this is precisely the state of affairs we expect to find with categorial reinterpretation. In other words, the Binding Theory and the Possessive Generalization requirements can only be met by assuming that in the competence of the speaker the NP headed by *vantaggio* 'advantage' does not function as such any more, at least in cases (24a-c). In this way the binding domain would be the whole sentence for both anaphors and pronominal elements thus justifying the judgements on (24b), and no input for the application of the Possessive Generalization would be available, thus allowing sentences (24a and c).

Of course, although an explanation of this kind seems to be generally correct - to state the existence of mental operations that modify the categorial status of certain strings constitutes a step towards a fuller understanding of what lies beyond the notion of idiomaticity - it is difficult to work out a detailed account of the operations involved in it, including justifications of why they can apply to nouns like *vantaggio* 'advantage' but not to others.[14]

I shall leave all these questions open, but, just to conclude this survey on the IPPs like *a vantaggio (di)* 'to (the) advantage of' and by way of simple illustration, let us assume that these IPPs undergo a sort of 'restructuring' rule, a device that has sometimes been used in generative research (even if with great caution) and which consists of an operation having the property of erasing unwanted categorial boundaries and of creating new ones.[15]

In the case of *a vantaggio (di)* 'to (the) advantage of' and other similar expressions, this operation would have the effect of erasing the typical nominal categorial projections of *vantaggio* 'advantage' (namely, NP and under an X-bar interpretation, N-bar - on the notions of X-bar syntax applied to the NP of Italian see Battye 1987). As a consequence of the erasure of the nominal categorial projections, *vantaggio* 'advantage' ceases to be a nominal head and creates a new zero level category X *a vantaggio* 'to (the) advantage' - the restructured sequence - which projects a maximal category XP, as shown in (43): [16]

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[14] Cf. Higginbotham (1985: 539ff) for observations in this connection made concerning the English idiomatic expressions - or 'semi-productive idioms' as he calls them - like *for someone's sake*.

[15] The restructuring rule has applied to so many different phenomena that it is hardly right to talk of an actual rule in the grammar or of the same rule for each phenomenon. The following is an indicative bibliography on the problem: Longobardi (1979), Manzini (1983), Pollock (1979), Rizzi (1976), (1982), Rouveret and Vergnaud (1980), Hornstein and Weinberg (1981), Zubizarreta (1980).

[16] An alternative account would be erasing the sole maximal projection of the N *vantaggio* 'advantage' thus leaving a reduced category - an N-bar type category in terms of the X-bar theory - which, besides being able to account for the Binding theory facts, could also explain extraction phenomena like **la persona di cui feci ciò a vantaggio* 'the person of whom I-did that to (the) advantage' vs. *la persona a vantaggio di cui feci ciò* 'the person to (the) advantage of whom I-did that' in terms of the 'minimality condition' of Chomsky (1986b). This solution, however, should be worked out in greater detail so as to meet the

(43) ... [$_{XP}$ [$_{X}$ a vantaggio] di NP]

Whatever lexical and categorial value is assigned to X, XP has to be assumed as differing from NP or S so as it may not be considered either a binding domain for anaphors and pronominal elements or an input to the Possessive Generalization, thus accounting for the judgements on the sentences (24a-c). As to sentences (24d-g), it only has to be assumed that the restructuring rule does not apply here, and that the categorial structure as represented in (26) remains intact. This assumption might sound too stipulative, but, in a sense, it is perfectly in keeping with the spirit of restructuring rules, whatever mental operations they may represent and whatever formal developments they may undergo in the future: restructuring should be thought of as an optional device which never affects the lexical component of a given language.

Of course more evidence would be necessary to support a detailed formal proposal about restructuring in cases like these. Surely, expressions like *a vantaggio di* 'to the advantage of' display different behaviours which may call for such an attempt. Some we have already observed at the beginning of this discussion. Others can be added as for instance the following contrast which results unexpectedly under normal analyses:

(44) a. *Di quale ditta l' affare fu fatto [a vantaggio t] ?
of which firm the business was done to (the) advantage

b. Di quale film hai assistito [alla prima
of which film you-have been present at the first

rappresentazione t] ?
showing

Possessive Generalization facts also. In particular, if we adopt it, we should have to assume an operation incorporating the *di* + pronominal/anaphor sequence into the N-bar category, thus treating *vantaggio* as an ergative nominal.

But examples like these suggest there are many possible ways to give a formal status to the idea of restructuring (cf. note 16 for a possible alternative to 43). It is then preferable to keep restructuring as a cover term indicating some sort of categorial modification in certain expressions - something that because of its exceptionality has to be demonstrated and not just hypothesized.

Thus, with this *mise-au-point* in mind, let us turn to the idea that restructuring may be optional. With regard to this it is worth noting that it correlates with adjectival modification in an interesting way: we saw that an adjective like *grande* 'great' cannot modify *vantaggio* 'advantage' when the latter is part of an IPP (cf. example 14 above). However when that IPP contains indications of the absence of Restructuring (i.e. the presence of a possessive adjective) modification with *grande* becomes more tolerable:

(45) a. ?Gianni aveva fatto ciò a suo grande vantaggio
Gianni had done that to his great advantage

b. Gianni aveva fatto ciò a grande vantaggio suo
Gianni had done that to great advantage his

With reference to optionality, it is also worth observing the behaviour of an IPP like *all'infuori (di)* 'with the exception of' which contains a nominal nucleus that does not occur in free contexts any more:

(46) a. Carlo odiava tutti all' infuori di se stesso
Carlo hated everybody with the exception of himself

b. *Carlo$_i$ odiava tutti all' infuori di lui$_i$
Carlo$_i$ hated everyone with the exception of him$_i$

c. Carlo odiava tutti all' infuori di lei
Carlo hated everyone with the exception of her

d *Carlo$_i$ odiava tutti al suo$_{i/j}$ infuori
Carlo$_i$ hated everyone with (the) his$_{i/j}$ exception

e. *Carlo$_i$ odiava tutti all' infuori suo$_{i/j}$
Carlo$_i$ hated everyone with the exception his$_{i/j}$

f. *Carlo odiava tutti al proprio infuori
Carlo hated everyone with his own exception

g. *Carlo odiava tutti all' infuori proprio
Carlo hated everyone with the exception his own

Sentences (46a-c) perfectly match sentences (24a-c) but sentences (46d-g), in contrast to (24d-g), are ungrammatical: this means that with an IPP like *all'infuori (di)* 'with the exception of' the restructuring rule applies obligatorily, but this exactly corresponds to saying that the sequence *all'infuori* 'with exception' belongs to the lexicon in spite of its (partial) analyzability, in other words, it amounts to saying that restructuring itself, in this case, is unnecessary.

The optionality of the restructuring rule, on the other hand, is able to rescue the descriptive and equivocal concept of crystallization to a certain extent: a restructured sequence, because of its resemblance to proper lexical items, is likely to disallow the presence of internal modifiers such as possessives, but a non-restructured one is not, a prediction that, although far from being a necessary one, seems to be perfectly attested in the paradigm (24) and in sentences (44a and b).

3.0 Conclusion

To sum up, the analysis of IPPs like *a vantaggio (di)* 'to (the) advantage of', though still containing many problems which are difficult to solve,[17] seems to answer positively the question we raised at the be-mysterious ones - with respect to pronominal and anaphoric possessives. Just to mention some of these, consider the following paradigms:

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[17] Besides the problems I have already sketched out, like those regarding the theoretical status of the restructuring rule or the relevance of the thematic structure of the nominals contained in the IPPs, there are many others concerning the behaviour of these IPPs with respect to other classes of phenomena or even concerning different types of behaviour - indeed

(i) a. Cesare$_i$ era stato molto ingiusto nei suoi$_{*i/j}$ riguardi
Caesar had been very unfair in (the) his regards

b. Cesare$_i$ era stato molto ingiusto nei riguardi suoi$_{*i/j}$
Caesar had been very unfair in (the) regards his

c. Cesare era stato molto ingiusto nei propri riguardi
Caesar had been very unfair in (the) his-own regards

d. Cesare era stato molto ingiusto nei riguardi propri
Caesar had been very unfair in (the) regards his-own

(ii) a. *Marco si ruppe una gamba per sua colpa
Marco broke (his) leg through his fault

b. Marco si ruppe una gamba per colpa sua
Marco broke (his) leg through fault his

c. *Marco si ruppe una gamba per propria colpa
Marco broke (his) leg through his-own fault

d. (?)Marco si ruppe una gamba per colpa propria
Marco broke (his) leg through fault his-own

(iii) a. *Pietro si ruppe una gamba a sua causa
Pietro broke (his) leg on his account

b. Pietro$_i$ si ruppe una gamba a causa sua$_{*i/j}$
Pietro broke (his) leg on account his

c. *Pietro si ruppe una gamba a propria causa
Pietro broke (his) leg on his-own account

d. ??Pietro si ruppe una gamba a causa' propria
Pietro broke (his) leg on account his-own

The indexing in (i) is similar to the one we found in restructuring sequences, but the presence of the possessive should exclude a restructuring rule such as the one we have been assuming. The indexing in (ii) perfectly matches the one we found in non-restructured sequences, but the ginning, namely, whether there is any sense in speaking of IPPs from a theoretical point of view. The idiosyncratic behaviour of these constructions with respect to the Binding Theory - a fully tested theory, at least in its essentials - compels one to assume some sort of categorial modification which corresponds to the suppression of the functional and lexical properties of the nominal head. This conclusion, although far from being an explanatory and exhaustive one, seems to be able to fix some important points for the empirical and formal identification of these idiomatic constructions and is a methodological step towards a theoretical definition of idiomaticity itself.

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impossibility of (ii) (a) and (c) seems to call for a sort of restructuring in any case. Finally, (iii) seems to incorporate the (contradictory) properties of both (i) and (ii), with the additional problem of the uncertainty of judgements on sentence (d).

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A SOLUTION TO THE 'MUST OF' PROBLEM*

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The 'must of' phenomenon is a familiar one to parents, lexicographers, pedants and developmental psycholinguists. It has been recognized since 1837 (see the numerous quotations in *OED Supplement* (1933) and *OED Supplement III: O-Scz* (1982)). It was at first considered 'US dialect or colloquial' (1933 Supplement), though this can scarcely be the way it is evaluated today. It consists of the utterance of the word *of* ([ɒv]) after the modals ***must, should, would, could, might*** and their negatives, and occasionally in perfective aspect infinitival complements (i.e. the ***I ought to have done it*** type), instead of 'adult', standard ***have***. Such utterances take place in one of three sets of circumstances:

(1) in accented clause-final position in elliptical utterances like *I haven't, anyway - Dad might of* (ex inf. JP); *I thought I'd turned it off, but I couldn't of* (ex inf. MB). Here it receives an unreduced form as it would if it were genuinely the preposition *of*; notice that 'real' ***have*** here would be unaccented and in an unreduced form.

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* Thanks for helpful comments are due to the Editors, to Gerald Gazdar and Roger Wales. Other people have provided material for me: thanks also to Maggie Boden, Maggie Moore, Trevor Pateman, Helen Petrie, Jennifer Platt and my hapless informants. Their examples are attributed by their initials in the body of the article. A preliminary version of this paper was published in *Aspects, Journal of the Language Society of the University of Sussex* 1 (2) (1987), 30-7, and I thank the editor, Faramarz Amiri, for permitting the material to be reused. The author's correspondence address is School of Cognitive Sciences, University of Sussex, Falmer, Brighton BN1 9QN, England.

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(2) when a young child in all circumstances uses unreduced spoken forms of all those English words which exhibit both reduced and unreduced forms:[1] as in *I could of done it* (child aged 6,9; ex inf. TP). (Note that children under the age of 7 are rarely reported as using *must* or certain other relevant modals.) This is occasionally done for obscure reasons by older people, thus *One thief must of been fighting the other* (XZ aged 12,3); *We really should of invited DD to dinner by now* (AB, adult; ex inf. MM). The examples cited here may no longer be checked, as one does not leave the tape-recorder going just in the hope of catching material of this kind. Thus it is not certain whether the speakers' use of unreduced *of* was in 'free' variation with the reduced form at the age and time at which they uttered it.

(3) when a literate person of any age writes *of* for the standard perfective aspect marker *have*, irrespective of the way they pronounce it: *I never would of married in the world* (1844); *I might of been glad when he went off with that bloody moll* (1946) (for these two see *OED Supplement III*, p. 24); *During the babbling period several sounds will of been used.....* (undergraduate essay, University of Q, 1986); ..... *hypotheses about why he might of acted in a certain way* (exam script, University of R, 1987; ex inf. JP); *this is the kind of description of results which should of.....* (a case reported as a self-corrected slip of the pen by HP); *I'd of liked* (cited as a type by Randolph Quirk in The Independent, 12/11/86); etc.

(Note that example sets (1)-(3) include every instance of this variable phenomenon that has come to my attention during the twelve months prior to submitting this paper; it seems to be common but elusive.)

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[1] In the case most familiar to me (XZ for much of the latter part of her third year), the articles were excepted from the list of items that occurred unreduced. The terms *(un)reduced* are understood in the familiar way without my being committed here to any particular viewpoint on the nature of the phonological processes involved.

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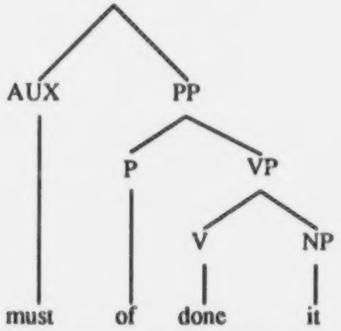
Those few writers who mention the matter, typically writers of prescriptive manuals, regard it as a 'gross solecism' and leave it at that (Partridge 1947 and every subsequent printing; Bailey 1976: 59). The phenomenon is usually 'explained' by saying that the form [əv] is susceptible of analysis as a reduced form of both *have* and *of*, and that utterers of the types of expression mentioned in (1)-(3) have chosen the wrong one (thus Fieldhouse 1982). But it is obvious that this is only the groundwork for a proper explanation. Why do errors of the type ***What ˈare you `thinking have?* not occur? And what licenses the interpretation of [əv] as *of* in syntactic environments where an aspectual marker rather than a preposition is apparently so obviously required? The latter is the crucial question. For if utterers of the relevant utterances interpret [əv] as *of*, they are arguably internally committed, in some sense, to analyses like (4), saving only the possibility of structure intervening between the nodes PP and VP, because of is quite unambiguously prepositional in all its other uses, even though it has a range of distinguishable senses.

(4)



(The highest node in this subtree is labelled following usual assumptions about the notion of headship, and nothing further is implied by the label.) But how on earth can it be that a preposition may have a VP complement? So far as I know there are only two published suggestions to this effect within modern grammatical traditions (Starosta 1977;

Emonds 1985: 89-90), although the view may be implicit in some other work (cf. Pullum 1982: 191, 194-5). Starosta's proposal, formulated within his 'lexicase' theory, relates to the 'complementizer' *to*, and the question of the status of *to* will be taken up again below. Emonds argues that [P VP] is a suitable analysis for certain s-structure gerunds introduced by complementizers, which in his theory are prepositions (1985: 281-332); in such structures VP is a transformational reduction of a sentence.[2] There are, of course, apparent instances of lexical prepositions in construction with VP in English (*keen on doing linguistics*), and therefore also of stranded prepositions in effect lexically representing PP/VP (a prepositional phrase with a verb phrase hole in it), e.g. gerunds in pseudo-cleft constructions (*What I'm keen on is doing linguistics*). A traditional Latinate grammatical model, and the reductionist approach of Hendrick (1978), would analyse the relevant phrases as NPs, however one might analyse the structure *within* the NP. But even if Emonds' analysis is correct, it is scarcely developmentally credible that such constructions could serve as a model for a child acquiring the complements of modal verbs that we are examining.

I shall argue now that trees like (4) instantiate a (NB not *the*) proper analysis of these phenomena, and allow readers to infer that some real children construct in their heads structures of precisely this type.

Our point of departure is the familiar view that linguists should aim at constructing the simplest analysis compatible with the phenomena under description, usually backed by the entirely questionable assumption that that is what real people (and even children) do. This requirement may lead to conflict between notions of simplicity applied in different sectors of the linguistic system. Thus since, in analyses other than Starosta's, prepositions regularly and uniquely govern NP in standard English, to admit trees of type (4) would reduce the simplicity of the syntactic description of prepositional phrases. *Done it* is clearly not an NP, e.g. it does not permit pseudo-clefting or other varieties of topicalization and cannot be pronominalized. On the other hand, it would clearly reduce the simplicity of the lexicon if we denied that the *of* in (4) really was an instance of the preposition *of*, in the sense that we might reasonably resist the admission of homonymous items to the lexicon except as a last resort.[3] Let us say that both of these analyses (a redundancy rule to the effect that P always governs NP, and a categorical statement that *of* is a preposition) are, in some sense, simplest analyses, and guess that the child aims to construct a simplest analysis, without our being able to predict, in individual cases, which one that will be.

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[2] Other proposals put forward for expanding the range of complements permitted to prepositions have concentrated on the possibility of [P S], e.g. Jackendoff (1973), van Riemsdijk (1978: ch. 3).

Let us now suppose that some children construct a simplest analysis of utterances involving [əv] as invariably containing of.[4] If they do, they need a grammar permitting structures like (4).[5] Consider the sentences in (5):

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[3] We do not need to search far for historical lexical changes which create homophony 'designed' to eliminate lexical obscurity, e.g. the replacement of *bridegoom* by *bridegroom*, where the last syllable can, as a result of the change, be interpreted as literally containing the more transparent element *groom*. Reinterpretations may trend the same way. Consider the product of accidental homonymy in the expression *ear of wheat*; this may, unhistorically, be seen as containing a metaphorical use of the ordinary word *ear*. For extensive discussion of similar things, see Coates (1987). Analyses which bring together senses of lexical elements which are apparently wildly at variance with each other are usually highly prized by linguists, and there is a rich anthropological literature on such matters. For just one, cf. Leach (1958), where the author seeks to reconcile the apparently disparate senses of the lexeme *tabu*, as used in Trobriand society, in rebuttal of Malinowski (1935: 28, 113); though against Leach see Chowning (1970). See also Grillo, Pratt and Street (1987: 277).

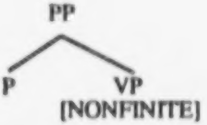
[4] I mean [əv], and not the [v] which clearly represents an auxiliary element contrasting with third-person singular [z], as in *I've/you've/she's/we've/they've*, which is a categorically vowelless enclitic, and which always seems to be analysed in this way.

[5] It might be possible to bolster the argument by adducing the analogy of gerundial forms after prepositions (cf. above), as in ((*I'm fed up) of doing this*, but as already noted the gerund is susceptible of other analyses than

(5) You ought to [tə] do it.
You must of [əv] done it.

Assuming that lexical simplicity demands, for this child, an unambiguous lexical category assignation for *to* as well, the child has a *prima facie* case for admitting constructions of type (6), since *to* has certain unambiguously prepositional functions,[6] as in directional phrases, indirect object phrases, etc.

(6)



Notice that *to*, like *of*, displays a full form ([tu]) and a reduced form ([tə]). Children who produce unreduced forms in all circumstances

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the one which makes it a VP, and is therefore a less convincing analogy; the traditional analysis as a NP appears valid from a distributional viewpoint. Of course I do not rule out the possibility that some developing speakers analyse this construction as [P VP]. At all events *done it* is far more unambiguously NOT an NP than *doing it*.

[6] Pullum (1982: 191-5) brings forward ten good arguments why 'complementizer' *to* is not a preposition in adult standard English, and I accept those arguments. His claim that *to* is an auxiliary verb does not of course entail that all occurrences of *to* are verbs. I am suggesting here only that an economical first hypothesis, by a learner or a linguist, might be that such a word was a unitary lexical item. In arguing against 'complementizer' *to* being a preposition, however, he states that 'no other prepositions.... take uninflected VPs as complements' (191). I hope to have shown here that a dialect of English exists where a preposition could be analysed as taking a nonfinite inflected VP. I note with interest the paper by Bloom, Tackeff and Lahey (1984), where the acquisition of 'complementizer' *to* is studied. The authors conclude that *to* is acquired first as a VP-complement marker, not as an infinitive marker, and that its usage appears to depend on a 'directional' interpretation, i.e. one which is prepositional *par excellence*. (The last inference ('i.e.....') is mine, not the authors'.)

((2) above) do so for both these prepositions. It is true that the distribution of the adult prototypes for the unreduced forms of *to* and what we are taking to be *of* (subsuming unaccented *have*) are not precisely identical, in that [tu] is required clause-finally, as is prepositional [ɒv], but not [hæv] as an auxiliary. But this item is exceptional in being the only unaccented item which is not a pronoun admissible in English in utterance-final position with a reduced vowel.[7] Those who say *I must of* (cf. example (1)) have ironed out this irregularity. I suggest therefore that despite this minor disparity in the behaviour of *to* and putative *of*, a prima facie case could be constructed that: just as *to* is characteristically a preposition; just as it occurs in a reduced pronunciation in determinable environments; just as it could be construed as subcategorizing for a nonfinite VP - so too does *of*. They differ, under this analysis, in that *to* subcategorizes for VP[NONFINITE, INFINITIVE] whilst *of* subcategorizes for VP[NONFINITE, PERFECT PARTICIPLE].

I have shown how it is possible to construct a case for the interpretation of [əv] in *must have done* (etc.) as the 'preposition' *of*, using principles which do not strike me as controversial even if the analyses to which they lead are.[8] Clearly if the learner eventually acquires standard English, and demonstrates this by writing *must have done* (etc.[9]), then he or she has replaced the rough-and-ready (but principled) guesswork of the solution offered here by one more that normally and traditionally considered to be appropriate for the mature standard dialect.

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[7] It is of considerable interest that one of my informants (YZ aged 5,10) began producing reduced forms of precisely *to*, in its complementizer function (*I don't want to*), and of no other 'preposition', in utterance-final position, thereby paralleling her now correctly acquired reduction of *have* in the same position. This suggests some kind of affinity betwen the two items for at least one maturing English-speaker. Cf. also Pullum (1982: 212, note 12).

[8] Bybee (1985: 42) argues that the grammatical force of the change from *have* to *of* is from a marker of aspect to one of tense. She is concerned to account for the 'fusability' (as she sees it) of the modal and the word in question, rather than to provide a grammatical analysis suitable for those lects in which it occurs. Her account does not appear to say anything about the significance of the substitution of *of*.

[9] This is likely to be the only evidence ever produced for the acquisition of the standard construction, as the conversational potential for the pronunciation of the full form [hæv] in expressions like *We should have*

It seems to be well established that it is in the nature of adpositions to govern NPs alone, i.e. this is a universal in the present state of our knowledge. If children and other learners are indeed able to construct theoretically impermissible analyses which violate universals, and even construct apparently absurd ones, then the consequences for universalist-nativist approaches to language acquisition are very interesting: the domain in which such approaches are deemed to have explanatory value must be reduced in proportion to the numbers of such constructions discovered.

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gone there is very limited indeed, except in over-careful reading aloud. Only neurotic doubt prevents me from asterisking it.

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THE 'NO CROSSING CONSTRAINT' IN AUTOSEGMENTAL PHONOLOGY*

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1 Introduction

Autosegmental Phonology is a theory of phonological representation which employs graphs rather than strings as its central data structure (van der Hulst and Smith 1982). Phonological processes such as assimilation, harmony etc. are given derivational accounts similar to those employed in string-based approaches to phonology. But in line with the trend towards declarative formalisms in linguistic theory, Autosegmental representations and derivations are sanctioned not by explicitly ordered grammar rules, but by general 'principles' tempered by 'constraints', together with some language-specific rules. Well-formedness of an Autosegmental representation and its derivation are assessed by their adherence to and satisfaction of these 'principles', 'constraints' and rules. In this paper we shall consider the principal 'constraint' of Autosegmental Phonology, the so-called 'No Crossing Constraint' (Hammond 1988), and show that

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it does not, in fact, constrain the class of well-formed Autosegmental representations. (See section 2 for a definition of the N.C.C.)

We are not alone in examining the basis of the No Crossing Constraint. Sagey (1988), for instance, examines two interpretations of the diagrams used in Autosegmental Phonology, and shows that the No Crossing Constraint follows as a necessary consequence of one of these interpretations. She concludes that the ill-formedness of Autosegmental representations with crossing association lines derives from extralinguistic knowledge about two timing relations, 'precedence' and 'overlap'.

In this paper, we consider Autosegmental Phonology as a theory of grammar for a particular family of graphical languages (sets of graphs yielded by graph-grammar derivations). We are careful to distinguish the syntax of these languages (i.e. the *form* of phonological representations) from their semantics, that is, from possible *interpretations* of those representations. The fact that the No Crossing Constraint can be derived from a particular interpretation of Autosegmental representations suggests that Sagey's hypothesis involves phenomena that are not strictly speaking extralinguistic, but rather 'extrasyntactic', i.e. semantic in the terms just defined.

As part of our work in constructing computational implementations of nonlinear phonology in the field of speech synthesis, we have independently duplicated Sagey's result, and have also developed the stronger syntactic argument that the No Crossing Constraint (N.C.C.) is not a constraint at all, strictly speaking, since it does not restrict the class of well-formed phonological representations. The core of our argument can be briefly sketched as follows:

A distinction must be drawn between Autosegmental phonological representations, and diagrams of those Autosegmental phonological representations. Diagrams are not linguistic objects, but pictures of linguistic objects, and may have properties such as perspective, colour etc. which are of no relevance to linguistic theory. The N.C.C. is a constraint on diagrams, not on Autosegmental representations. When the conditions by which the N.C.C. restricts the class of diagrams are examined and linguistically irrelevant factors such as width or straightness of lines are removed, it is apparent that the *intention* of the N.C.C. is to enforce the following *planarity constraint*: Autosegmental representations are planar graphs. Thus, the planarity constraint is the defining distinction between the two varieties of Autosegmental Phonology, planar and nonplanar (i.e. multiplanar).

In planar Autosegmental Phonology the No Crossing Constraint has no place in *linguistic* theory, since it is *universally* the defining characteristic of planar graphs. In nonplanar Autosegmental Phonology the N.C.C. is unrestrictive, because *all* graphs can be drawn as 3-D diagrams with no lines crossing.

We consider our syntactic argument to be stronger than Sagey's semantic argument, since it is not dependent on a particular theory of the interpretation of phonological representations, but follows from general principles of graph theory alone.

The rest of this paper is set out as follows. In section 2 we consider the syntax and semantics of Autosegmental representations and we introduce a few important basic definitions and principles of graph theory. In section 3 we consider the veracity of the claim that the No Crossing Constraint restricts the class of representations in planar and nonplanar Autosegmental Phonology, and we examine the belief of some proponents of 3-D Autosegmental Phonology that the *necessity* of nonplanar representations has already been demonstrated. Finally, in section 4 we present some data exemplifying a number of interacting harmonies in Guyanese English that are amenable only to nonplanar representation.

2 Autosegmental Phonology and Graph Theory

We begin by considering the syntax and semantics of Autosegmental phonological representations, introducing definitions of the terminology which we employ in our subsequent argument. Initially, we must be careful to distinguish Autosegmental representations (A.P.R.s), which are linguistic objects, from both diagrams (which are pictorial objects) and graphs (which are mathematical objects).

Let us first consider the question 'What are A.P.R.s?' A naïve answer to this question is that they are diagrams i.e. pictures in journals, etc. This first hypothesis can easily be dismissed. Being pictorial objects, diagrams are necessarily flat. However, diagrams may have properties, such as perspective, that are not shared by phonological representations. For instance, it is possible to portray a two-dimensional object on a flat surface but with a three-dimensional perspective, e.g. by drawing a circle in 3-space as an ellipse in the plane of the paper. (We shall refer to perspectiveless diagrams as 2-D diagrams, and to diagrams with three-dimensional perspective as 3-D diagrams.) Therefore, diagrams in journals are not in themselves Autosegmental representations, but pictures of Autosegmental representations. What then, are Autosegmental representations?

A more sophisticated hypothesis, which does not fall prey to the immediate problems of the naïve hypothesis, is 'a phonological representation is a mathematical object that has precisely the 'important' diagrammatic properties that phonological diagrams have.' But this hypothesis begs the question as to which diagrammatic properties are 'important', and which are not. The resolution of this question is fundamental to this paper.

In Autosegmental Phonology, a phonological representation consists of a number of phonological objects (segments, autosegments and timing slots) and a two-place relation, called *association* (A), over those objects. In addition, the phonological objects in an Autosegmental representation are partitioned into a number of well-ordered sets, called *tiers*.

In Autosegmental *diagrams*[1], phonological objects are represented by alphabetic symbols, features or vectors of features, and the association relation by straight lines connecting each pair of objects that is in the association relation. Tiers are portrayed in Autosegmental diagrams by horisontal sequences of objects separated by spaces. The No Crossing Constraint is the statement that in a well-formed Autosegmental diagram, lines of association may not cross.

We shall not consider what A.P.R.s denote. A large number of views concerning this question have been advanced over the years, and it seems unlikely to us that agreement will ever be reached.

Despite the ongoing debate about the semantics of A.P.R.s, it is possible to demonstrate our claims concerning the nonrestrictiveness of the No Crossing Constraint from consideration of the *syntax* (i.e. form) of A.P.R.s alone. In order to do this, we first set out some elementary definitions and theorems of graph theory.

In mathematics, a collection of objects and a two-place relation defined over those objects (often with the explicit inclusion of endpoint maps π_1 and π_2, though these are usually omitted if multiple arcs are not permitted, cf. Rosen 1977) is called a *graph*.

Formally, a graph G is a tuple (V, E), (optionally with the addition of endpoint maps π_1, π_2) where V is any set of objects, called *vertices* in graph theory, and E a set of pairs of vertices, called *edges*.

The term 'vertex' is a general mathematical term for primitive objects in whatever domain is being modelled, and the term 'edge' is a mathematical term for each pair of objects in a relation. The *degree* of a vertex is the number of edges of which that vertex is a member.

The definition of a graph and the terminology of graph theory are completely independent of any particular drawing conventions that may be used to represent a graph diagramatically. Graphs are abstract mathematical entities with no unique visible manifestation. In particular 'graph' is not synonymous with 'diagram', 'vertex' is not synonymous with 'point' and 'edge' is not synonymous with 'line'. We must thus be careful to distinguish graphs from their various possible diagrammatic instantiations. Vertices can be any type of object whatsoever, edges are simply pairs of vertices, and graphs are simply pairs of sets of vertices and edges.

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[1] Figures (2) and (3) are typical Autosegmental diagrams.

A *bipartite* graph is one in which the set of vertices can be partitioned into two disjoint subsets N_1 and N_2, such that the set of edges is a subset of $N_1 \times N_2$. Since in Autosegmental Phonology the association relation holds only over objects on separate tiers (i.e. an object may not be associated with any other object on the same tier), the existence of tiers in Autosegmental Phonology ensures that its graphs are bipartite. For instance, N_1 might be the set of timing slots, and N_2 the set of melody segments.

Graphs in which the set of nodes can likewise be divided into n disjoint subsets are called n-partite graphs. An Autosegmental representation with an anchor tier and m melody tiers is thus *maximally* an $(m+1)$-partite graph, though it might, depending on what conditions are imposed on the association relation, be *minimally* merely a bipartite graph. This is the case if melody units can only be Associated to anchor units, and not to each other. Pulleyblank (1986: 14) advances this condition. It is also the case if the theory of tier organisation proposed by Clements (1985), in which each segment is a tree of melody-units, makes Autosegmental representations at most bipartite (since all trees are bipartite graphs). In multi-tier 2-D Autosegmental diagrams, association lines do not usually 'cross' tiers except at a node. The graphs denoted by these digrams are also (at most) bipartite. Since m-partite graphs where $m > 2$ are therefore prohibited in one way or another from occuring in the several varieties of Autosegmental Phonology, Autosegmental diagrams denote (amongst other things) bipartite graphs.

A complete bipartite graph of the form $K_{1,s}$ is called a *star* *graph*. Such a graph has one node (the *root*) linked to each of the others (the *leaves*).

A *circuit* graph is a connected graph in which every node is of degree two. We define a *chain* to be a circuit graph with one arc removed. In a chain, every node is linked two two others except for two end-nodes, which are of degree one.

An *Autosegmental Phonological Representation (A.P.R.)* is a triple $(G, L, \prec)$, where G is a graph (O, A, π_1, π_2) of the association relation, O is the set of phonological objects, L a partition of O, A a subset of $L^i \times L^j$ where L^i and L^j are members of L, $i \neq j$, π_1 and π_2 maps from A to O which pick out the endpoints of each association line, and $\prec$ a total order on each L^n.

In his 'Excursus On Formalism', Goldsmith (1976: 28) defines an Autosegmental phonological representation as a set of sequences L^i of objects a^i_j (each of which is a tier, which Goldsmith calls 'levels'), together with an ordered sequence A of pairs of objects whose first and second members are drawn from disjoint tiers. Apart from the total ordering of elements in the association relation A, the characterisation of Autosegmental phonological representations which we presented in the preceding paragraph is identical to Goldsmith's.

Our first line of attack on the N.C.C. is to show that it follows directly from Goldsmith's explicit total ordering of A.

Let $\trianglelefteq$ be the total ordering on A, let $\{(a, b), (c, d)\} \subset A$ and let $\{a, c\}$ and $\{b, d\}$ be in disjoint tiers. Goldsmith states (1976: 28) '*A* in a sense organizes the other levels' (i.e. the endpoints of A). Although it is not completely clear what he means by this statement, we claim that $(a, b) \trianglelefteq (c, d) \Leftrightarrow a \prec c$ and $b \prec d$. For if this is not so, the total ordering on A serves no purpose and should be dispensed with.

Theorem 1 *The N.C.C. follows from the total ordering of A.*

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Proof 1 *If 'lines cross' (i.e. if $a \prec c$ but $d \prec b$) then neither $(a,b) \trianglelefteq (c,d)$ nor $(c,d) \trianglelefteq (a,b)$, in which case $\trianglelefteq$ is not total and A is not well-defined. A is only well-defined if lines do not cross, or in other words, if the N.C.C. holds.* □

But if the N.C.C. can be derived so trivially from mathematical properties of Autosegmental representations, it is not a *linguistic* constraint.

However, since the total ordering of association lines is an undefended stipulation of Goldsmith's, we shall proceed in the argument which follows in the belief that the N.C.C. is not vacuous, therefore abandoning the stipulation that A is totally ordered.

Since all Autosegmental phonological representations are graphs on which some further restrictions have been placed, all of the *universal* properties of graphs hold of Autosegmental phonological representations, together with some *special* properties. Autosegmental representations are a special kind of graph, but they are also subject to all the universal properties of graphs.

Our formal characterisation of A.P.R.s differs from Goldsmith's (1976: 88) attempt at formalisation only in the ordering of A. Our characterisation correctly captures all the necessary structural properties of Autosegmental representations, (division into tiers, well-ordering of tiers, adjacency and locality of neighbouring elements within a tier, the association relation), but Goldsmith's attempt to derive the N.C.C. from preservation of connected subsequences of segments under inversion of the association relation fails to work in a number of elementary cases, as he admits in two footnotes (Goldsmith 1976: 55, notes 5 and 6).

Having established that Autosegmental representations are graphs (a kind of formal object), we shall consider the relationship between the two kinds of Autosegmental diagrams (2-D and 3-D), and two kinds of graphs, planar graphs and Euclidean (nonplanar) graphs.

2.1 Planarity

A *Jordan curve* in the plane is a continuous curve which does not intersect itself. A graph G *can be embedded* in the plane if it is isomorphic to a graph drawn in the plane with points representing the vertices of G and Jordan curves representing edges in such a way that there are no crossings. A *crossing* is said to occur if either

1. the Jordan curves corresponding to two edges intersect at a point which corresponds to no vertex, or
2. the Jordan curve corresponding to an edge passes through a point which corresponds to a vertex which is not one of the two vertices which form that edge.

A *planar* graph is a graph which can be embedded in a plane surface.

A *Euclidean* graph is a graph which can be embedded in Euclidean space, that is, normal, three-dimensional space. All planar graphs are Euclidean, but not all Euclidean graphs are planar. That is, there are some Euclidean graphs which cannot be embedded in the plane.

The two kinds of graphs and diagrams we are considering are expressed in the following table:

Graphs:	Planar graphs	$\subset$	Euclidean graphs
Diagrams:	2-D diagrams	$\subset$	3-D diagrams

We have singled out the planar/Euclidean distinction for particular consideration, since it might be thought that there is a simple one-to-one relation between planar graphs and 2-D diagrams, and Euclidean graphs and 3-D diagrams. We shall demonstrate that this is not the case, and that this mistaken view underlies a number of problems with the N.C.C.

By definition, every planar graph can be drawn in the plane of the paper as a flat or perspectiveless network of points and noncrossing lines (a 2-D diagram); and every flat network of points and noncrossing lines represents a planar graph.

By definition, every 3-D network of points and noncrossing lines represents a Euclidean graph. We now show that the reverse case also holds.

Theorem 2 *Every graph can be embedded in 3-D space.*

Proof 2 *We shall give an explicit construction for the embedding. Firstly, place the vertices of the graph at distinct points along an axis. Secondly, choose distinct planes (or 'paddles') through this axis, one for each edge in the graph. (This can always be done since there are only finitely many edges.) Finally, embed the edges in the space as follows: for each edge joining two distinct vertices, draw a Jordan curve connecting those two vertices on its own 'paddle'. (We assume there are no edges joining a vertex to itself.) Since the planes or 'paddles' intersect only along the common axis along which all the vertices lie, none of the Jordan curves corresponding to the edges of the graph cross.* □

Theorem 3 *Every graph G can be drawn in a 3-D diagram as a network of points and (in perspective) noncrossing lines.*

Proof 3 *Embed G in 3-D space. Draw G in perspective.* □

The fact that in Autosegmental representations, the set of vertices is partitioned into tiers, each of which is totally ordered, does not affect the validity of these Theorems. The single axis of vertices required for the construction used in the proof of Theorem 2 may be partitioned into subsets of well-ordered objects without affecting the result.

2.2 Paddle-wheel Autosegmental representations

Pulleyblank (1986: 12–14) considers limitations on the association relation. He argues that if the objects in every tier may only be associated with the objects (‘slots’) in a distinguished (‘skeletal’) tier, and not to the objects in any other tier, the theory which results is ‘considerably more restrictive’. The graphs yielded by Pulleyblank’s proposed restriction have come to be known as ‘paddle-wheel’ graphs (Archangeli 1985: 337) since they consist of a set of planar graphs which intersect along a shared tier, the skeleton (cf. 3 and 14 below). Since Autosegmental representations are graphs, Pulleyblank’s claim must mean that versions of Autosegmental phonology which allow only ‘paddle-wheel’ graphs are ‘considerably more restrictive’ than theories which allow general graphs. Yet Theorem 2 shows that this is not correct. For Pulleyblank’s claim to hold, there must also be restrictions on the composition of each tier other than linear ordering (in other words, restrictions on the *objects* in each tier), and on the *straightness* of association lines.

A number of Autosegmental phonologists (Clements and Keyser 1983: 11, Prince 1984: 235, Clements 1986, Pulleyblank 1986: 14) who subscribe to the ‘paddle-wheel’ theory claim that timing relations between Autosegments are dependent on the ordering of objects in the skeleton. In this case, the maximally parsimonious account is one in which autosegmental tiers are not explicitly ordered.[2] If only elements on the skeletal tier are ordered, then the N.C.C. has no force since

```
a b        b   a
 \/    =   |   |
 /\        |   |
C  V       C   V
```

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[2] Kaye (1985: 289,301–304) crucially requires nonskeletal tiers to be unordered, as does Lowenstamm and Kaye (1986), although this latter paper explicitly denies that phonology is three-dimensional, despite accepting the principles of Autosegmental phonology.

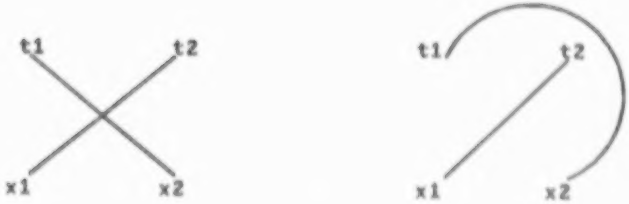

Figure 1: Two embeddings of a planar graph.

where C and V are on the skeletal tier and the tier $\{a, b\}$ is unordered.

The need for association lines to be straight for the N.C.C. to work can be demonstrated as follows. Consider the graph:

$$(\{t_1, t_2, x_1, x_2\}, \{(t_1, x_2), (t_2, x_1)\})$$

with partition into tiers $T_1 = \{t_1, t_2\}$, $T_2 = \{x_1, x_2\}$ and the order $t_1 \prec t_2$, $x_1 \prec x_2$ (1a). If the N.C.C. requires association lines to be straight, then this Autosegmental representation cannot be drawn without crossing lines (1a), and it would thus be *excluded* by the N.C.C. But if there is no such restriction on the straightness of association lines, this Autosegmental representation *can* be portrayed without crossing lines (1b), and thus the N.C.C. does not prohibit this A.P.R.

This demonstrates that the No Crossing Constraint is a condition on pictures, not phonological representations, since straightness of lines is a property of pictures, not linguistic representations. The straightness of association lines is conventional rather than formal: it has never been explicitly defended in Autosegmental Phonology, it does not follow from other principles of the theory, and it is sometimes abandoned when it is convenient to do so (see for example McCarthy (1979/1982: 140), Archangeli (1985: 345), Prince (1987: 501), Pulleyblank (1988: 256,259), Hayes (1989: 300)). If the lines denoting the association relation need not be straight, then the N.C.C. will sometimes necessarily hold and at other times only contingently hold. The cases in which the N.C.C. contingently holds are those like (1), in which if the lines need not be straight, the N.C.C. can be circumvented. In such cases, the N.C.C. is nonrestrictive, and therefore cannot be linguistically relevant. However, in the cases in which the N.C.C. *necessarily* holds, it is indeed restrictive, for it limits the class of Autosegmental representations to planar graphs. In these cases adoption of the N.C.C. is equivalent to support for the hypothesis that Autosegmental representations are planar graphs.

Since the straightness of association lines is a property of Autosegmental diagrams, and not Autosegmental representations, Pulleyblank's position can only be maintained if there are constraints on tier composition which would diminish the force of our criticism. No such constraints have yet been established, although there are several possibilities:

1. Each tier bears a bundle of features, each of which cannot occur on any other tier.
2. Each tier bears a single phonological feature.
3. Each tier bears all of the segmental structure dominated by a single node of the Universal segment tree. (Clements 1985)
4. Each tier represents a morpheme. (McCarthy 1979/1982, Halle and Vergnaud 1980)

The first position cannot be maintained, since it is necessary in Autosegmental Phonology to allow more than one tier to bear *the same* feature or features. Such proliferation of tiers has been employed in Autosegmental analyses of cases where a single feature (or set of features) has two different morphophonological functions. Prince (1987: 499) gives the following illustration of this:

> Arabic requires the same features to appear on different planes: for example, the affix /w/ is featurally identical to any other /w/, yet it clearly stands apart, tier-wise, because a root consonant may spread over it without line crossing in form XII

Halle and Vergnaud (1980) contains many examples just like this Arabic case.

Yet without the prohibition against the multiplication of features on different tiers, this position is simply the unconstrained null hypothesis that Autosegments are (unconstrained) bundles of features.

The second position (the 'single feature hypothesis', or S.F.H.) has been challenged on the grounds that it is empirically inadequate: it is sometimes desirable to treat two or more features as a single Autosegmental unit (when they have the same distribution, for instance). McCarthy's widely-supported analysis of Semitic morphology requires entire segmental melodies, not just single features, to be Autosegmental. The single feature hypothesis would not be sufficient to maintain Pulleyblank's claim concerning the restrictiveness of paddle-wheel A.P.R.s, unless multiplication of single features on several tiers (a move which is necessary to Autosegmental Phonology) were also prohibited.

The third hypothesis, proposed in Clements (1985), also falls foul of the need identified by McCarthy (1979/1982) and Prince (1987) for feature-structures to be replicated on several tiers. In every case, such replication undermines the restrictiveness of *any* proposal regarding tier composition, Clements, (1985) included.

The fourth hypothesis, the 'morphemic plane hypothesis', or M.P.H., is not sufficient to maintain Pulleyblank's claim, because it begs the question as to what phonological objects may constitute a morpheme. McCarthy (1989) shows that in the analysis of some languages (e.g. Mayan) it is necessary to represent vowel and consonant features on independent planes, although there is no evidence that vowels and consonants constitute separate morphemes.

We have thus shown that

- Autosegmental representations are graphs;
- 'paddle-wheel' graphs are no more restricted than general graphs;
- this fact is not diminished by the partition of objects into well-ordered tiers;
- no formal constraints on membership of phonological objects in tiers have yet been established.

From these, it follows that Pulleyblank's claim regarding the relative restrictiveness of 'paddle-wheel' A.P.R.s and Euclidean A.P.R.s is incorrect.

Let us conclude our consideration of the relationships between diagrams and graphs. We have shown that every planar graph can be drawn as a 3-D diagram. *However, not every 3-D diagram represents a planar graph. Some 3-D diagrams represent necessarily nonplanar graphs.* In order to understand the N.C.C., we are especially interested in the class of properly (i.e. necessarily) nonplanar graphs, which cannot be drawn in 2-D diagrams without crossing lines.

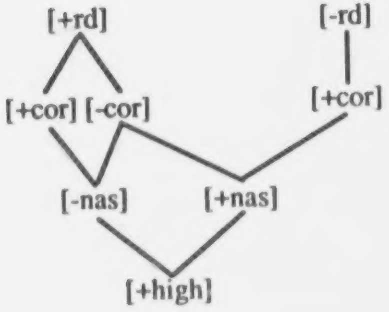

Figure 2: A planar graph.

3 Planarity and the N.C.C.

In the early days of Autosegmental Phonology (Goldsmith 1976), all Autosegmental diagrams were drawn as if to lie entirely in the plane of the paper. As we showed in the preceding section, however, if the No Crossing Constraint applies to Autosegmental representations, not diagrams, it defines a general, topological sense of planarity: namely, (planarity condition) a graph is topologically planar if and only if it can be embedded in a plane surface with (by definition of 'embedding') no edges crossing. Not all graphs can fulfil this requirement, however they are drawn, and it is therefore necessary to determine whether all Autosegmental representations can, if only in principle, be drawn in the plane. If some cannot, then A.P.R.s are in general nonplanar (whether they are portrayed as such or not), and the No Crossing Constraint is not restrictive.

If the N.C.C. applies to diagrams, it is a drawing convention, not a part of linguistic theory. But if it applies to Autosegmental representations, the planarity condition and the No Crossing Constraint are equivalent: the No Crossing Constraint has no specifically linguistic status, in that it is the defining characteristic of planarity, and should therefore be dropped from phonological analysis. We foresee that proponents of the N.C.C. might wish to argue that if the N.C.C. is retained, it is the planarity condition which is vacuous. But this argument is inadequate in two respects. Firstly, in addition to prohibiting Autosegmental representations which are necessarily nonplanar, the N.C.C. also excludes Autosegmental representations which may be portrayed in diagrams whose lines only contingently cross, even if there is some other way of drawing them in which no lines cross. Thus the N.C.C. prohibits some Autosegmental representations merely on the basis of the way in which they might sometimes be portrayed. The planarity condition, on the other hand, is a condition on Autosegmental representations, not diagrams of Autosegmental representations. It thus constrains linguistic (phonological) representations, not diagrams of linguistic representations. Secondly, to the extent that 'no crossing' is a universal property of planar graphs and not just those planar graphs employed in phonological theory, there is no reason for the mathematical definition of planarity to be imported into linguistics as a 'principle of Universal Grammar'. The relevant principle is 'Autosegmental representations are planar graphs' (if such is the case), not 'association lines must not cross'. For these reasons, given the equivalence of the N.C.C. and the planarity condition, those Autosegmental Phonologists seeking to defend the planarity hypothesis would do better to adopt the planarity condition directly (since it is a constraint on linguistic representations) than the N.C.C., which is a constraint only on *drawings* of linguistic representations.

Various phonologists (e.g. Archangeli 1985, Goldsmith 1985, Pulleyblank 1986) have found it convenient to generalize the planar representations of early Autosegmental phonology to 3-D, 'paddle-wheel', representations, in which several independent Autosegmental planes intersect along a distinguished tier of timing units. There is,

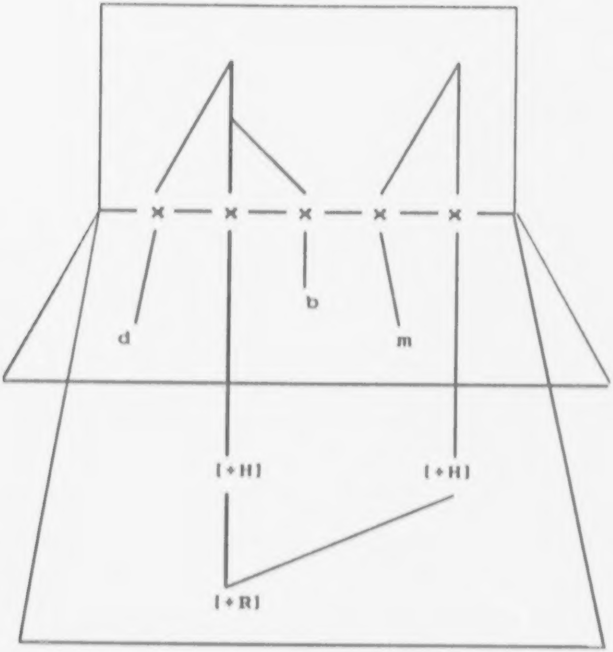

Figure 3: A paddle-wheel graph.

however, an important distinction between convenience and necessity. It is widely believed and commonly assumed by Autosegmental Phonologists that the *necessity* of 3-D representations has already been uncontentiously demonstrated. Archangeli (1985: 337), for instance, writes

> McCarthy's (1979;1981) analysis of Semitic *forced a truly three dimensional* phonological representation. [Our emphasis.]

Yet McCarthy (1979;1981) contain not a single diagram which even *appears* to be nonplanar, let alone a *necessarily* nonplanar representation.

Because the belief in the necessity for nonplanar Autosegmental representations is widespread, it has rarely been defended in the literature. As far as we are aware, no necessarily nonplanar phonological representation has yet been presented as a proof that Autosegmental representations are nonplanar.[3]

We shall attempt to defend our claim that the nonplanarity of Autosegmental representations has not yet been proven by establishing a necessary and sufficient criterion for a graph to be (necessarily) nonplanar. We shall then use this criterion to test the logic of the argument and examples adduced in support of the claim that phonological representations have already been shown to be necessarily nonplanar. We shall argue that the falsity of claims in the literature about 3-dimensionality arise from a failure to distinguish diagram conventions from genuine and uncontentious universal properties of graphs.

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[3] In our examination of the literature, we have discovered one case of a diagram of an Autosegmental representation which is, in fact, demonstrably nonplanar. But this example is not offered in defence of the argument that Autosegmental representations are nonplanar. It is presented as part of a derivational account of timing in Ancient Greek. We discuss this case further below.

It is harder to show that a graph is *necessarily* nonplanar than that a diagram is 3-D. For a diagram to be 3-D it merely has to appear to be 3-D. A necessary and sufficient criterion for the nonplanarity of a graph G is:

Theorem 4 *(Kuratowski 1930)*[4] *G is nonplanar if and only if it contains a subgraph which is homeomorphic to either of the two graphs K_5 (the fully connected graph over five vertices) and $K_{3,3}$ (the fully connected bipartite graph over two sets of three vertices), shown in figure (4).*

(Two graphs are *homeomorphic* if they can both be obtained from the same graph by inserting new vertices of degree two into its edges.)

Note that Kuratowski's Theorem requires only that a subgraph of a graph is shown to be nonplanar in order to show that the whole graph is nonplanar.

Since the association relation is a bipartite graph, homeomorphism to $K_{3,3}$ is a necessary and sufficient condition for an Autosegmental representation to be necessarily nonplanar.

3.1 3-D Diagrams in the Autosegmental Literature

Figure (3), taken from Archangeli (1985), is typical of those diagrams of the association relation that are purported to be necessarily nonplanar. Archangeli's logic is inexplicit, but seems to be as follows (cf. Archangeli, 1985: 337): suppose there is a tier above the anchor tier (for instance, a consonant melody), and another tier below the anchor tier (for instance, a vowel melody). Then there are at least two 'paddles', one in the plane of the paper above the anchor tier,

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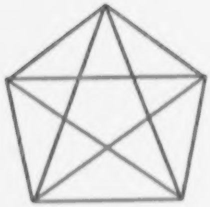
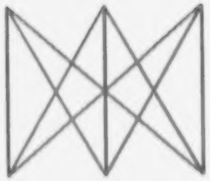
[4] It is beyond the scope of this paper to present a proof of Kuratowski's theorem, since it is requires extensive and advanced familiarity with graph theory. A reasonably approachable presentation of the proof is Gibbons (1985: 77-80).



K5



K3, 3

Figure 4: Nonplanar graphs.

the other in the plane of the paper below the anchor tier. Now if yet another independent tier is called for (syllable templates, perhaps), yet another paddle, separate from the two in the plane of the paper, is required. Thus phonological representations with more than two melody tiers on separate paddles are necessarily nonplanar. This argument is erroneous. Figure (3) has three independent paddles, but it is not homeomorphic to either K_5 or $K_{3,3}$, and can be drawn in the plane with no lines crossing. Figure (5) is one possible plane embedding of (3). All the other examples of three-paddle Autosegmental diagrams that we are aware of from the literature (with the exception of the one we discuss below) also have plane embeddings. The graph of which figure (5) is a plane embedding is in no way affected by the manner in which it is portrayed. Since it is unchanged, it retains all the structure of figure (3), still supporting reference to all the relevant notions of locality (adjacency) and accessibility as in figure (3). Such an embedding is nothing other than a different way of looking at the same formal object.

The argument which Archangeli offers is, as far as we are aware, the only published attempt to establish the nonplanarity of Autosegmental representations. However, Archangeli's hypothesis has been generally accepted by Autosegmental phonologists, presumably because it is undeniably *convenient* to use 3-D diagrams in Autosegmental Phonology. We will examine some more examples of 3-D diagrams taken from the literature and show why, like Archangeli's example, they are not *necessarily* nonplanar.

Pulleyblank (1988). Like Halle and Vergnaud (1980), Pulleyblank (1988) subscribes to the M.P.H., stating:

> Following McCarthy (1981;1986) among others, I assume that the melodic content of different morphemes enters the phonology on distinct tiers (planes).

But Pulleyblank's examples contain at most only two morphemes, and thus all of them represent graphs which may be embedded in the

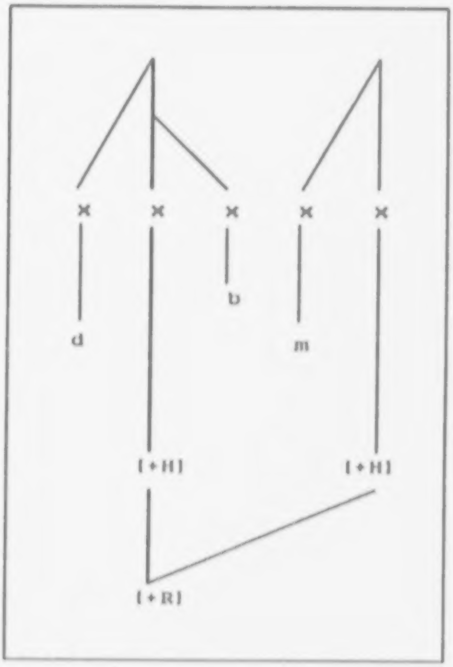

x
x
x
x
x
d
b
m
[+H]
[+H]
[+R]

Figure 5: A planar embedding of a paddlewheel graph.

plane. None of Pulleyblank's 'apparently' 3-D diagrams (44), (50) represent *necessarily* nonplanar graphs.

Halle and Vergnaud (1987). Halle and Vergnaud argue that Autosegmental representations consist of several intersecting planes, and that they are therefore 'three dimensional' (by which, since they are talking about linguistic representations, not diagrams, we assume they must mean nonplanar). The rhetorical structure of their argument makes liberal use of conjunctions such as *thus, since, in fact* and *therefore* to build the appearance of a logically coherent, incremental argument, but this impression is deceptive because their argument is defective in several respects. Because it is one of the few papers in which an argument for the 3-D nature of Autosegmental representations is explicitly presented, we shall go through it very carefully, emphasizing the unsupported conclusions.

> Autosegmental phonology has made it clear that tones must be represented as a sequence of units (segments) that is separate and distinct from the sequence of phonemes — in other words, that in tone languages phonological representations must consist of two *parallel* lines of entities: the phonemes and the tones. (Halle and Vergnaud 1987: 45).

The conclusion that the sequence of phonemes and the sequence of tones are *parallel* is unsupported. It is true that in Autosegmental diagrams, tiers always are parallel, but no Autosegmental phonologist has ever even attempted to demonstrate that 'phonological representations *must* consist of two parallel lines'.

> Since two parallel lines define a plane, we shall speak of the *tone plane* when talking about representations such as those in (1). (Halle and Vergnaud 1987: 45).

Two parallel lines do indeed define a plane, but Halle and Vergnaud have not established that associated tiers are parallel.

The next step in Halle and Vergnaud's argument is to show that stress, like tone, is autosegmental.

> We propose to treat stress by means of the same basic formalism as tone — that is, by setting up a special autosegmental plane on which stress will be represented and which we shall call the *stress plane*. (Halle and Vergnaud 1987: 46).

> It is not an accident that the bottom line both in the tone plane and in the stress plane is constituted by the string of phonemes representing the words. In fact, all autosegmental planes intersect in a single line, which as a first approximation may be viewed as containing the phoneme strings of the words. Autosegmental representations are *therefore* three-dimensional objects of a very special type: they consist of a number of autosegmental planes (to be geometrically precise, half-planes) that intersect in a single line, the line of phonemes. (Halle and Vergnaud 1987: 46).

This displays the same false reasoning as Archangeli (1985), discussed above. The establishment of several independent tiers linked to a common core is not sufficient to *prove* that A.P.R.s are *necessarily* nonplanar. It *is* sufficient to motivate the use of 3-D diagrams for clarity of presentation, but expository convenience is not a relevant factor in assessing the nature of phonological representations.

> We have argued that stress is represented on a separate plane from the rest of the phonological structure. It has been proposed elsewhere that other properties of morphemes, such as tone (Goldsmith 1976) and syllable

> structure (Halle 1985), are also to be represented on separate planes. Therefore, a morpheme will in general be represented by a family of planes intersecting in a central line. Given this formalization, the combination of morphemes into words will involve a combination of families of planes. (Halle and Vergnaud 1987: 54).

Even if we grant that the tiers in an A.P.R. are parallel, and therefore do indeed define a family of planes, it does not follow that such a family of planes defines a three-dimensional object. While it is undoubtedly conceptually and pictorially *convenient* to picture a family of planes as forming a three-dimensional object, it is geometrically quite possible for a family of planes to lie in the same planar space.

Halle and Vergnaud even fabricate supporting evidence for their conception of phonological structure. They claim that:

> McCarthy (1986) has proposed that the separate autosegmental planes of Semitic morphology are the result of the fact that distinct morphemes must be represented on separate planes — for example, as in (20). (Halle and Vergnaud 1987: 54)

But unlike Halle and Vergnaud (1987) and Goldsmith (1985), McCarthy's (1986) article contains no autosegmental representations that are even *apparently* nonplanar (and no 3-D diagrams), let alone *necessarily* nonplanar. The 3-D diagram which Halle and Vergnaud credit to McCarthy is their own, not McCarthy's.

The structure of Halle and Vergnaud's argument can be summarized as follows:

1. Autosegmental tiers are parallel to the skeleton.
2. Therefore, each tier defines a (half-)plane.

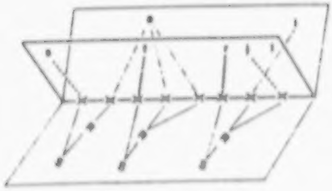


Figure 6: A 3-D diagram.

3. An autosegmental representation may contain several autosegmental tiers.
4. Therefore, an autosegmental representation consists of a family of intersecting (half-)planes.
5. Therefore, an autosegmental representation is a three-dimensional object.

Their argument does not go through, however, since the first proposition is unsupported and the final conclusion does not follow from the premisses.

Halle (1985). Although 3-D diagrams are rare, even in the writings of such proponents of '3-D Phonology' as Halle and Vergnaud, Halle (1985) presents a 3-D diagram, a representation of the Arabic word *safaarij* 'quinces' (6). However, no subgraph of the graph represented in this diagram is homeomorphic to $K_{3,3}$ or K_5, and thus (6) portrays a planar graph.

Halle (1985) is clear that diagrams such as (6) are not to be confused with the A.P.R.s that they denote. He states:

> information about the phonic shape of the words is stored in a fluent speaker's memory in the form of *a three-dimensional object that for concreteness one might picture* as a spiral-bound notebook. (Halle 1985: 101, our emphasis).
>
> I have tried to present *a picture* of this type of representation in Figure [6]. (Halle 1985: 112, our emphasis).

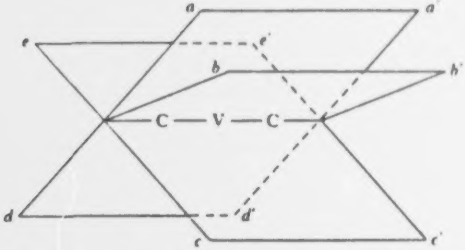
Moreover, Halle is clear that the diagrams of '3-D Phonology' are a *notation* for Autosegmental Phonological Representations, rather than being the representations themselves:

> there are no promising alternative *notations* to the multi-tiered autosegmental representation that has been described here. (Halle 1985: 112, our emphasis).

Yet, as we have argued throughout this paper, arguments for the felicity or utility of 3-D diagrams, or in other words, pictures of A.P.R.s do not constitute evidence the necessary nonplanarity of those representations.

Clements (1985). Clements (1985) examines two models of segmental organisation.

> To clarify our ideas, it would be useful to contrast two possible models of multi-tiered feature representation, representing opposed views of hierarchical organisation. (Clements 1985: 227)



aa' = sonorant tier, *bb'* = continuant tier, *cc'* = high tier,
dd' = back tier, *ee'* = voiced tier

Figure 7: 3-D diagram with skeletal core.

In the first model, a segment is a star-graph whose root node is a skeletal object, whose leaf nodes are autosegments, and whose edges are association lines. The sequence of leaf nodes in adjacent segments forms tiers, and the sequence of root nodes forms a skeletal tier (7). Phonological representations are thus:

> multi-tiered structures in which all features are assigned to their own tiers, and are linked to a common core or 'skeleton'. (Clements 1985: 227)

Clements uses the metaphor of an open book[5] to describe such graphs:

---

[5] Halle is fond of the ring-bound notebook metaphor.

> In such a conception, a phonological representation resembles an open book, suspended horizontally from its ends and spread open so that its pages flop freely around its spine. The outer edge of each page defines a *tier*; the page itself defines a *plane*, and the spine corresponds to the *skeleton*. (Clements 1985: 228)

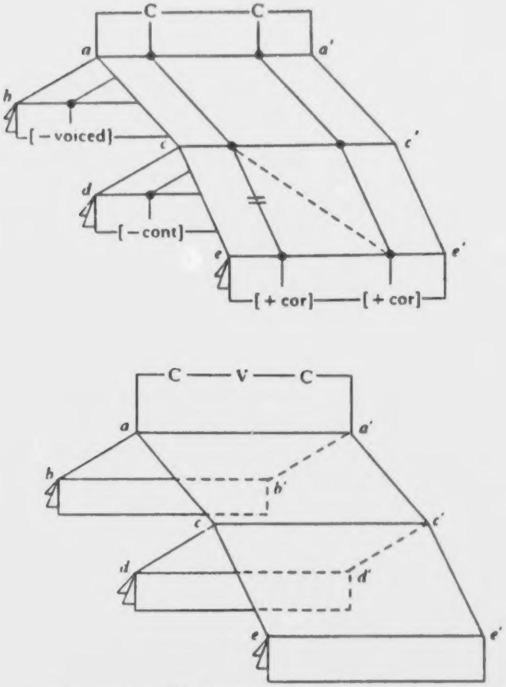
Each segment in this model is a star-graph consisting of a skeletal slot linked to the features which constitute that segment, each on its particular tier. The linear extension of a star-graph is a 'paddle-wheel' graph.

Clements contrasts this view with an alternative model in which each segment is not a star-graph but a tree-graph (8).

> This conception resembles a construction of cut and glued paper, such that each fold is a class tier, the lower edges are feature tiers, and the upper edge is the CV tier. (Clements 1985: 229)

Like other writers, Clements provides a number of appealing arguments for using 3-dimensional diagrams, and indeed offers empirical evidence in support of his position. But nowhere does he demonstrate that the evidence he musters explicitly proves that autosegmental representations are necessarily nonplanar graphs. All that he demonstrates is that it is convenient for expository reasons, simplicity etc. for A.P.R.s to be multiplanar objects of a particular type.

Both of the models which Clements compares are capable of supporting the Single Feature Hypothesis of tier content, but the second, tree-structured model is not capable of supporting the Morphemic Plane Hypothesis, as it is a highly specific theory of tier content. To the extent that morphemes may be phonologically arbitrary, in the manner described by Prince (1987: 449) and discussed above, it is inadequate as a constrained theory of noncatenative morphology.


C
C
a
a'
b
[− voiced]
c
c'
d
[− cont]
e
e'
[+ cor]
[+ cor]
C
V
C
a
a'
b
b'
c
c'
d
d'
e
e'

aa' = root tier, *bb'* = laryngeal tier, *cc'* = supralaryngeal tier,
dd' = manner tier, *ee'* = place tier

Figure 8: 3-D diagrams with tree-structured segments.

There are, of course, many theories of segmental organisation consistent with all the principles of Autosegmental Phonology, other than the two which Clements singles out for consideration. Goldsmith (1976: 159), in which segments are *chains* of Autosegments, or Pulleyblank (1986: 13),[6] in which noncore tiers may be associated to other noncore tiers, are two attested alternatives, and others are possible.[7]

Goldsmith (1985). In this paper, Goldsmith employs 3-D diagrams for the first time. (It is not clear whether Goldsmith, Halle or Clements was first to do this. In each cases, the earliest publication of 3-D diagrams was in 1985.) However, Goldsmith (1985) uses 3-D diagrams for expository purposes only, and makes no theoretical claims about them. He states:

> The seven vowels of Mongolian are the seven vowels that can be created by the combinations of the feature [*front*] (represented as [i]), the feature [*round*] ([u]), and the feature [*low*] ([a]). These combinations arise through the association of a skeletal position with segments on three distinct tiers, on for each of these three features. This is illustrated in [*fig. 9*], where I have attempted to use perspective to represent four distinct tiers. (Goldsmith 1985: 257)

In his conclusion, he states:

> if the spirit of the analyses of Khalkha Mongolian, Yaka, Finnish, and Hungarian that are presented here is fundamentally correct, then the revisions of our conception

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[6] Pulleyblank discusses, but does not subscribe to this view.

[7] For instance, segmental structure might quite plausibly represented by directed acyclic graphs, as in Unification Phonology, *circuits* or *wheels* (star-graphs with leaf-to-leaf association cf. Wilson, 1985: 19).

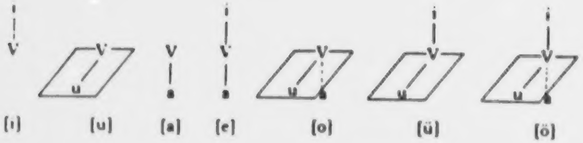

Figure 9: The vowels of Khalkha Mongolian.

> of phonological representation that we must adapt to are far-reaching, affecting both our view of autosegmental geometry and our understanding of traditionally segmental features. We will have to come to grips with truly rampant autosegmentalism (Goldsmith 1985: 271)

But unlike Halle and Clements, Goldsmith does not claim that A.P.R.s are three-dimensional objects.

Pulleyblank (1986). There are no necessarily nonplanar graphs in Pulleyblank (1986), although he does present a few considerations on the topology of Autosegmental representations. Like Halle and Vergnaud, Pulleyblank takes the view that:

> Nasality may be represented on a separate tier, vowel harmony features may be autosegmentalized, etc. This means that a language may require several independent (but parallel) tiers in its phonological representation. (Pulleyblank 1986: 12)

Just like Halle and Vergnaud, Pulleyblank slips in the unsupported assertion that if several independent tiers are required, they must be parallel, an assumption which is crucial to the hypothesis that tiers are organised into planes.

Pulleyblank considers two types of nonplanar Autosegmental representations. The first possibility which he considers is that each tier may be associated to any other. The only *formal* argument which Pulleyblank gives for rejecting this view is that tier-to-tier association can lead to contradictions in the temporal sequencing of autosegments. Commenting on figure (10), his example (21), he says:

> In (21), segments A and C have the value E on tier *p*; segment B, on the other hand, has the value F by virtue of the transitive linking B — D — F. But note that F *precedes* E in (21a), while it *follows* E in (21b)! In other words, the representation in (21) has as a consequence that the temporally ordered sequence EF is nondistinct from the sequence FE. (Pulleyblank 1986: 13)

This argument is flawed because association is not a transitive relation. If association *were* transitive, then the temporal interpretation of contour segments and geminates would be logically paradoxical (Sagey 1988: 110).

Furthermore, if temporal sequence is determined by the order of core elements, nonskeletal sequence is redundant. If nonskeletal tiers are unordered, then the apparent problem which Pulleyblank identifies vanishes.

Pulleyblank proposes an alternative type of nonplanar representation, paddle-wheel graphs, by adopting the restriction that Autosegmental tiers can only link to slots in the skeletal tier. He claims that the effect of this constraint is a 'considerably more restrictive

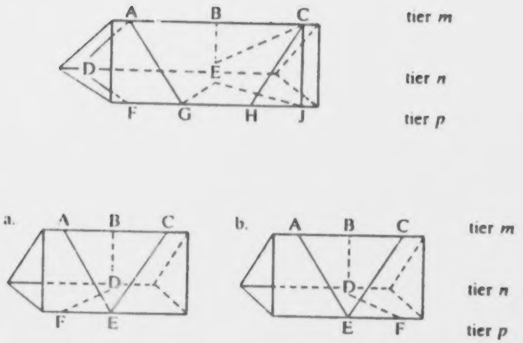
A
B
C
tier m
D
E
tier n
F
G
H
J
tier p
a.
A
B
C
D
F
E
b.
A
B
C
tier m
D
tier n
E
F
tier p

Figure 10: Direct tier-to-tier linking.

multi-tiered theory', a claim which we challenged above.[8] The only example of a 3-D diagram of a paddle-wheel graph which Pulleyblank presents includes no association lines at all, and it is therefore (trivially) planar.

McCarthy (1981). McCarthy (1981) includes none of the apparently 3-D diagrams of his earlier thesis (McCarthy 1979/1982), although the material in this paper is an abridged version of parts of that work. The framework is that of *n*-tiered autosegmental phonology without organisation into planes à la Halle and Vergnaud. In fact, quite contrary to Halle and Vergnaud, McCarthy has diagrams such as (11) (McCarthy 1981: 409 fig. 53) in which the CV 'core' occurs twice, in order to show the morphological correspondence between the first binyam and pə?al?al forms. (This is not a phonological representation, but a declarative formulation of reduplication.)

Halle and Vergnaud (1980). Although Halle and Vergnaud do not present any 3–D Autosegmental diagrams, they argue that 'the phonological representation is a three dimensional object' (Halle and Vergnaud 1980: 101) in the following manner.

> Its core is constituted by a linear sequence of slots — the skeleton. Each morpheme of the word is represented by a sequence of distinctive feature complexes ...the MELODY. (Halle and Vergnaud 1980: 101)

They accept the proposals of Autosegmental phonologists concerning the conditions which govern the linking of melody tiers to the skeleton, and claim that:

---

[8] The theory which results is considerably more *restricted*, but that is a different matter. Generative grammars of a particular type are certainly made considerably more restricted if their nonterminal symbols must all be words over the Cyrillic alphabet, but no more restrictive for all that.

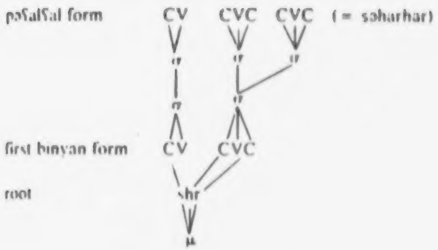

Figure 11: A diagram with two CV 'cores'.

> The lines that link the melody with skeleton define a plane. Thus, the phonological representation of a word contains as many planes as there are morphemes in the word. (Halle and Vergnaud 1980: 101)

This argument suffers from the same logical fallacy as that of Archangeli (1985): the (undisputed) clarity of presentation afforded by drawing subsets of the association relation over each individual morpheme's melody and the skeleton does not amount to a *proof* that planar Autosegmental representations are formally inadequate. Furthermore, as we argued above, without restrictions on what phonological material can constitute a morpheme, the departure from planar representations which Halle and Vergnaud support diminishes the force of the N.C.C. to the extent that it ceases to be restrictive.

McCarthy (1979/1982). McCarthy's (1979/1982) thesis extended Goldsmith's (1976) Autosegmental theory of tonal phenomena to the nonconcatenative morphology of Semitic languages. There are no diagrams in this thesis which are even apparently 3-D, and nowhere in the text is the possibility of multiplanar (as opposed to multi-tiered) representations raised, although six of McCarthy's examples might, with generosity, be taken as attempting to portray Autosegmental graphs using perspective. These are reproduced in (12). Even if these examples are taken to be 3-D diagrams, they do not portray nonplanar graphs. Since they are all drawn on a plane surface with no crossing lines, they all portray planar graphs.

Goldsmith (1976). Goldsmith (1976) concentrates on two-tier Autosegmental representations, those with just a phoneme tier and a tone tier. He considers extending this formalism to multi-tiered Autosegmental representations (of which he presents a planar example portrayed in a 2-D diagram), but does not raise the possibility of 3-D diagrams or nonplanar Autosegmental representations.

We have shown that the logic which underlies the common belief that a necessarily nonplanar Autosegmental representation already exists is mistaken. This is surprising, for under the conventional assumption concerning the universality and homogeneity of language, demonstration that just one Autosegmental graph is necessarily nonplanar is necessary and sufficient for rejection of planar Autosegmental Phonology and 2-D diagrams, as inherently too restrictive.[9]

One such graph has in fact been portrayed in the Autosegmental literature (there are perhaps others too), in Wetzels (1986). In

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[9] This is exactly parallel to a case from the history of Context-Free Phrase Structure Grammars. Throughout the 1960s and 1970s it was believed and taught by grammarians that Chomsky (1963: 378–379) and others had proved that English was not a Context-Free Language. In the early 1980s, however, these 'proofs' were shown to be fallacious in various respects (Pullum and Gazdar 1980), and it was not until some years later that respectable proofs of this widely-believed fact were actually constructed (Manaster-Ramer 1983, Huybregts 1984, Shieber 1985, Culy 1985).

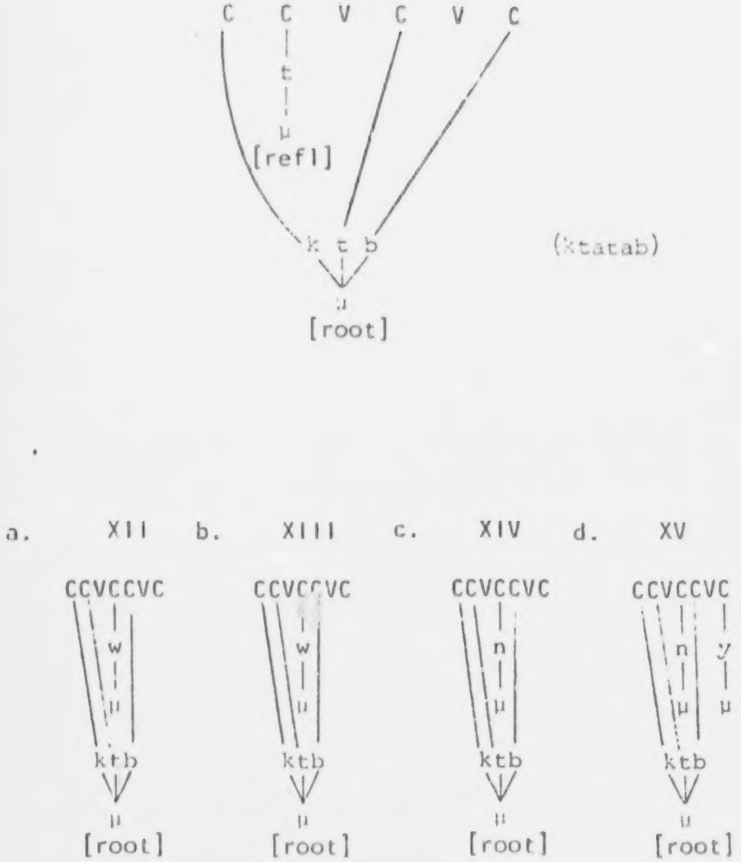
C C V C V C
t
μ
[refl]
k t b
μ
[root]
(ktatab)
a. XII
b. XIII
c. XIV
d. XV
CCVCCVC
w
n
y
μ
ktb
[root]

Figure 12: Autosegmental representations of Arabic.

the course of an Autosegmental derivation Wetzels gives a few diagrams of Autosegmental representations which contain $K_{3,3}$ as a subgraph. However, Wetzels's example is not presented as a proof that Autosegmental representations are nonplanar. Since he does not remark on the fact that his examples are nonplanar graphs, he appears to believe that the nonplanarity of Autosegmental representations has already been established. Furthermore, since Wetzels's example is from Classical Greek, and is therefore not amenable to first-hand verification, and since his analysis may be called into question, as a demonstration of nonplanarity it is not as uncontentious as is desired for a result to be established. We shall therefore present a synchronic example of an Autosegmental representation which is *necessarily* nonplanar. We shall establish the necessity of 3-D diagrams in Autosegmental Phonology by presenting an Autosegmental representation which is homeomorphic to $K_{3,3}$.[10]

4 A Necessarily 3-D Diagram

Consider a phonological representation with three anchor units on one tier, three autosegments on one or more other tiers, and a line of association between each anchor and each autosegment. Such a graph cannot be drawn without crossings on a plane surface, since it is homeomorphic to $K_{3,3}$.

There is no linguistic reason why such a representation might not be motivated in certain cases. Wetzels's example (13) is one such case. Two more are illustrated in (14), which shows the distribution of backness, rounding and nasality over three timing units in the pronunciation of the words 'room' and 'loom' by a Guyanese English speaker. Both of the graphs portrayed in (14) are homeomorphic to $K_{3,3}$, and thus they are necessarily nonplanar. In order to demonstrate that the graphs portrayed in (14) are the correct

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[10] Whether we are successful or not in advancing this example, however, does not affect our general argument concerning the vacuity or nonrestrictiveness, as the case may be, of the N.C.C.

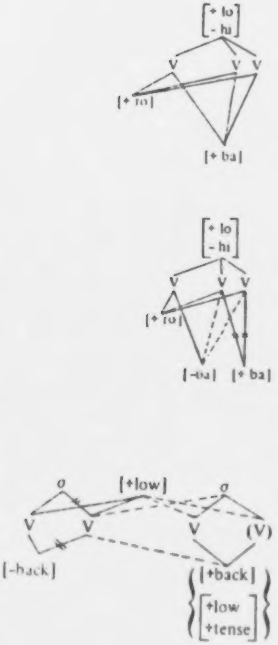
[+lo -hi]
V
V
V
[+ro]
[+ba]
[+lo -hi]
V
V
V
[+ro]
[-ba]
[+ba]
σ
[+low]
σ
V
V
V
(V)
[-back]
[+back]
[+low +tense]

Figure 13: Nonplanar Autosegmental representations.

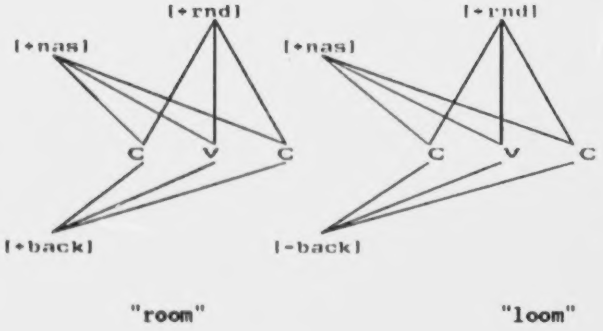
[+rnd]
[+nas]
C
V
C
[+back]
"room"
[+rnd]
[+nas]
C
V
C
[-back]
"loom"

Figure 14: Necessarily nonplanar Autosegmental representations.

Autosegmental representations of the two words, we must establish that the features [*back*], [*round*], and [*nasal*] are indeed independent autosegmental features. We shall demonstrate that this is so by showing that they are lexically associated with independent segments, and therefore must spread independently. For this to be the case, they must lie on independent tiers. Before we demonstrate this, we shall briefly explain the way in which application of rules to Autosegmental representations is notated in Autosegmental Phonology.

The two basic representation-altering operations of Autosegmental Phonology are the addition of association lines to Autosegmental representations and the deletion of association lines from Autosegmental representations. Association lines which are added to a representation are drawn as dotted lines. Thus (16f) and (17c) denote representations to which a single association line has been added, and (16a–e), (17a,b) and (18) denote representations to which two association lines have been added. Where the addition of association lines to a representation incrementally 'links' a single item on one tier to successive objects on another tier, the single item is said to 'spread'. There are no instances of deletion in this example, so we shall not discuss it further.

We shall argue in detail that comparison with similar words of the same general phonological shape, such as 'tomb', 'root', 'loot' and so on, shows that the spread of backness, rounding and nasality is clearly phonologically distinctive, and cannot simply be attributed to automatic phonetic coarticulation effects. Consider the transcriptions in (15).

4.1 Rounding

Along with the proponents of Autosegmental Phonology, we regard it as uncontentious that there is a feature of liprounding (under whatever name) which is a primary articulation of vowels and a secondary articulation of consonants. A comparison of (15f) with (15a–e) shows that the spread of rounding from rounded vowels to neighbouring

a) room	[ʋ̫̃̄ũm̫̄]
b) loom	[l̫̃'ũ̟m̫']
c) zoom	[z̫ɵ̠̃ũm̫]
d) root	[ʋ̫̄ɵ̠ut̫̄]
e) loot	[l̫'ɵüt̫']
f) lute	[l'ëüt̫']
g) rule	[ʋ̫̃̄ũl']
h) feud	[f'j̥ëüd̄<]
i) cube	[k̟j̥ëüb̄<]
j) rip	[ʋ̃̄ɩp']
k) red	[ʋ̃̄ɛd̄<]

Superscript ' and - denote palatalized and velarized articulations respectively. Superscript < denotes ingressive airstream (implosion), subscript $_{o}$, $_{+}$ and $_{-}$ respectively denote voicelessness, advanced and retracted articulations.

Figure 15: Transcriptions of Guyanese English words.

consonants is not an automatic coarticulatory effect, but is a phonologically principled phenomenon.

Comparison of (15a), in which the coda is rounded, with (15g), (15h) and (15i), in which the codas are not rounded even though a rounded nucleus precedes, demonstrates that Coda rounding is not an automatic coarticulatory effect, but is a phonologically principled phenomenon. In accordance with Autosegmental Phonology's preference for autosegmental analyses of feature-spreading, the perseverative rounding of the coda in (15a) must be attributed to the spreading of the autosegmental feature [*rnd*].

There is no *phonetic* reason why rounding should not spread from the second vocalic element in (15f) to the first vocalic element and thence to the initial consonant. Consequently, something must block the spread of rounding to the onset of (15f). There is no reason to regard the onset itself as the locus of this blocking: (15b) and (15e) show that rounding is sometimes found with palatalized lateral onsets. Thus it must be the first vocalic element which blocks the forward spread of rounding in (15f). The only nonarbitrary way of blocking such a spread in within the terms of Autosegmental Phonology is to propose the presence of an adjacent autosegment which is associated to the skeletal tier in such a way that the N.C.C. would be violated if the spreading continued further. Thus it is not possible to derive (16g) from (16f). (This analysis also demonstrates that [*rnd*] is Autosegmental even if there are two V units in the Autosegmental representation of (15f).)

4.2 Nasality

A comparison of (15c) with (15a) and (15b) shows that nasality spreads from nasal coda consonants to vowels (an uncontentious analysis) and thence to 'liquid' sonorants /l/ and /r/. The absence of nasality in the onset of 'zoom' (as well as 'soon', which behaves similarly) shows that this spreading is phonologically conditioned (17).

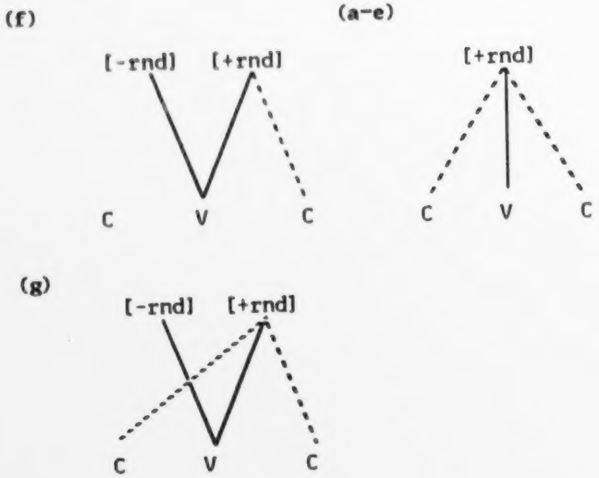
(f)
[-rnd]
[+rnd]
C
V
C
(a-e)
[+rnd]
C
V
C
(g)
[-rnd]
[+rnd]
C
V
C

Figure 16: Rounding is autosegmental in Guyanese.

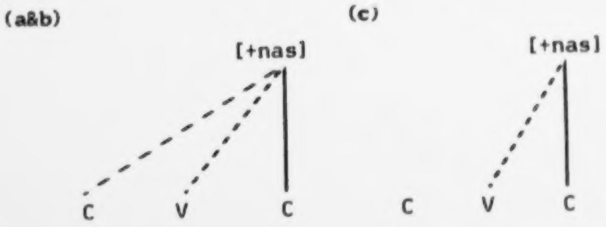

Figure 17: Nasality is autosegmental in Guyanese.

4.3 Backness

A comparison of (15a) with (15b) and (15d) with (15e) shows that the 'liquid' onsets /l/ and /r/ are systematically associated with the feature [±*back*], as is characteristic of many varieties of English (cf. Kelly and Local 1986, 1989: 74, 1989: 218-241). In this variety, /l/ is [−*back*] and /r/ is [+*back*]. Although the secondary articulation of onset consonants (notably obstruents) in English is attributable to the features of the vocalic nucleus, this is not the case with liquid onsets. The [−*back*] liquid remains [−*back*] before systemically [+*back*] vowels (15b) and the [+*back*] liquid remains [+*back*] before systemically [−*back*] vowels (15j,k). The distinctive association of liquid onsets with [±*back*] affects the nucleus and coda too, resulting in advanced vowel qualities and palatalized codas with the 'clear' liquid (15b), and retracted vowel qualities and velarized codas with the 'dark' liquid (15a). The spread of [±*back*] as far as the coda only applies in the case of coda consonants which are not lexically associated to [±*back*]. In the case of liquid codas, of course, spreading of [±*back*] from the nucleus is sometimes blocked (15g). Thus

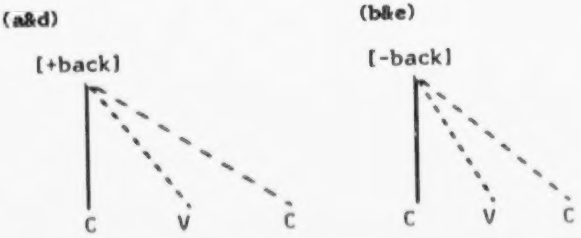

Figure 18: Backness is autosegmental in Guyanese.

[±*back*] is an autosegmental feature of liquids which in (15a) and (15b) spreads from the onset to the nucleus and thence to the coda (18). In the terms of Autosegmental Phonology, it is clear in the analysis of (15a) and (15b) that

- [±*rnd*], [±*back*] and [±*nas*] must be autosegments on separate tiers;
- liquid onsets are lexically associated with [±*back*], the nucleus with [±*rnd*], and the coda with [±*nas*]; and
- these three autosegments then spread to each of the other syllable terminals, as in (15a–e), (17a,b), and (18) to produce the Autosegmental representations portrayed in (14).

These interacting principles are each widely exemplified in several varieties of English, and although we present only a handful of critical examples here, many more may be found in Kelly and Local (1986, 1989).

Planar graphs are not in general adequate for Autosegmental representations of Guyanese English, because the Autosegmental representations of 'room' and 'loom' cannot be planar. Given that the principles which interact to produce this result are not particularly special and are individually attested elsewhere, we have no reason to believe that Guyanese English is either unnatural or special in this respect, and thus planarity (i.e. the No Crossing Constraint) is too severe to be a universal constraint on Autosegmental graphs.

Since Autosegmental Phonology is necessarily nonplanar, the No Crossing Constraint has no force, because all graphs, however complex, can be drawn in three dimensions without edges crossing. (The fact that some versions of Autosegmental Phonology employs 'paddle wheel' graphs, rather than unrestricted (i.e. Euclidean) graphs, does not affect this result.)

We conclude that the No Crossing Constraint is not a valid constraint at all, since it either incorrectly restricts the class of phonological graphs to planar graphs, or else it carries no force.

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PHRASE STRUCTURE, POSSESSIVES AND DEFINITENESS

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Definiteness and indefiniteness are usually seen as essentially a matter of lexical semantics, in that whether an NP is definite or indefinite depends on the choice of determiner. I want to suggest in this paper that there may be more to it than this - that the position of determiners (using this term as a non-theoretical convenience, neutral with respect to category status) within phrase structure configurations may correlate with the definite-indefinite distinction.[1] The idea is that definite and indefinite determiners occupy different positions in the NP, and that, furthermore, some constituent which is not lexically a definite determiner can induce definiteness in an NP by occupying the relevant position in it. The view is widely held that at LF indefinites are open, while definites are quantificational, a variable in N' (taking NP to be N") being bound by some specifier. My proposals amount to the idea that this structure is mirrored in the syntax, and thus run counter to the observation of Barwise and Cooper (1981: 201) that semantic distinctions between determiners have no syntactic correlates in NP structure. I use the term *specifier* standardly to denote, for English, the pre-head position daughter to NP, and my starting point is the claim that definite determiners occur in this position as members of the category Det, while indefinite determiners, which have been argued to be always cardinal, for instance by Löbner (1984), are generated as daughters to N' in what I shall call *modifier* position.

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[1] A first version of this paper was presented at the Fifth Groningen Round Table, 1984, and formed the basis of talks given at the Universities of Manchester and Essex in 1985. I am grateful for the many valuable comments made at each of these meetings. Author's address: Department of Modern Languages, University of Salford, Salford, M5 4WT.

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Notice first that many of the indefinite or weak determiners (such as *two, many, few*) can also occur in definite NPs, and always follow the definite Det (*the two/many/few men*), though some others, like *a*, cannot. I would claim that these expressions in themselves have nothing to do with indefiniteness, and that *the many* cannot be treated as a sort of compound definite determiner, as proposed by Keenan and Stavi (1986), since it is not a constituent. I take it to be uncontroversial that definite Dets occur in the specifier, and it has been widely accepted in work on NP structure that *two, many, few* occur within N'. In particular, two of the most substantial studies to date of NP phrase structure, Jackendoff (1977) and Selkirk (1977), give detailed arguments for such an analysis; see also Klein (to appear) for an adjectival treatment of cardinality terms. Jackendoff and Selkirk both assume that when these items are not preceded by a definite Det, it is best, for the sake of generality, to take them to be still in what I call modifier position. There is a possible problem for this analysis with the 'indefinite article' *a*, since this determiner is never preceded by a definite Det and in fact only occurs in indefinite NPs. But I believe this can be accounted for by a phonological constraint, for which there is considerable cross-linguistic support, to the effect that unstressed elements (such as *a* and *the*) can only occur phrase-initially; therefore whenever the specifier is filled, this blocks the occurrence of *a* in the following modifier position (or perhaps *a* is deleted - I leave open the precise mechanism by which *a* is prevented from appearing).

The reason that specifier position is restricted to definite determiners is that the specifier plays a special role in the interpretation of NPs. The N' contains a set expression, while the specifier is the position for expressions which impose a restriction relating to the domain of interpretation. Thus in a sentence like *the books are boring*, the intersection of the set of books and the set denoted by the VP must be non-null, and in addition must be identical to the set of books. Thus the definite Det in the specifier relates the set given by the intersection to the total set of books in the domain. An expression like *many* would not be expected to occur in specifier position because its lexical meaning does not correspond to such a restriction. It is true that *many* can have a quantificational reading in addition to the more usual cardinal one, as pointed out by Milsark (1979: 199-205); thus *many books* can be understood as equivalent to *many of the books*, *many* relating the intersection of the N' set with the predicate to the total set of books in the domain. And, as Milsark observes, *many books* with this reading does not readily occur in existential sentences, and is thus aligned with definites. But I think Milsark's suggestion that there is a class of indefinite determiners, including numerals, for which this quantificational reading is not available, is mistaken. Milsark is tentative on this matter, but it seems clear that in a sentence such as *three students went hiking, the other two remaining behind*, the numeral *three* is understood quantificationally. It can be claimed, then, that all cardinal determiners have available an additional quantificational interpretation.[2] There are two possible ways of incorporating this fact into my analysis. The first is to say that when these determiners have the quantificational reading, they then occur in the specifier; this would make them definite, which is reasonable if resistance to occurring in existential sentences is taken as a central criterion for definiteness. The second approach, which I will adopt, is to assume that cardinals are always in modifier position, because they are not always quantificational. On this view, while the appearance of a determiner in specifier position necessarily correlates with a quantificational reading, the possibility is admitted of a determiner with a quantificational reading not being in the specifier; this seems acceptable given that such a reading is available, though perhaps rather marked, for any cardinal determiner. I will assume, then, that specifier position is limited to those determiners which are always quantificational. Expressions occurring within N' are either part of the set expression, or a predicate over the intersection (as in the case of cardinality terms). Thus, in *the three big houses are too expensive*, the adjective *big* forms part of the set expression of N', limiting the set to that of

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[2] The only determiner that cannot be quantificational is *a*, and I suggest this is simply because *a* is unstressed; in fact it has often been argued that *a* is the weak, reduced form of *one*. Now it is a fact (noted by Milsark) that quantificational and cardinal *many*, *some* do not behave identically with respect to stress; the determiner must be stressed (and thus have full, unreduced vowels) to get the quantificational reading. With this qualification added to the claim that all cardinals can be used quantificationally, the exceptional position of *a* follows automatically.

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big houses rather than houses; the cardinality term *three*, also in N', is a predicate over the intersection of the set of big houses and the set of too expensive things; and the Det *the* in the specifier ensures that this intersection set is identical to the set of big houses.

Now, elements which contribute to the set expression by further delimiting the set denoted by the N (adjectives, etc.) normally occur within N', but it follows from my thesis that if such an expression were to occur in specifier position, it would have the effect of imposing a constraint relating to the domain comparable to that imposed by a definite Det such as *the*. In general such occurrences are not found, presumably because there are Dets available to serve the purpose and the proper place for a descriptive expression is in N'. But there is one important exception. In English, possessives and NPs in the genitive (I shall use the term *possessive* to cover both) occur in specifier position (*the/those/the teacher's/our three old books*). And NPs containing a possessive specifier must necessarily be understood as definite. Thus *John's books, a teacher's work* have the sense of *the books belonging to John, the work done by a teacher*, respectively, not *some books..., some work...* This is in spite of the absence of a definite Det; possessives are sometimes described as definite determiners, but this is valid only as an informal way of noting that NPs with a pre-head possessive are necessarily definite. The point is that possessives are not lexically definite; this is particularly clear with a phrase like *a teacher*, which makes *a teacher's work* (with genitive Case assignment) definite, equivalent to *the work of a teacher*. It might be argued that definiteness is inherent in the genitive affix *'s*, but this view would be hard to maintain in face of the fact that possessives can be clearly indefinite when predicative (as *Ann's* in *this book is Ann's - she writes her initials in all her books*). It is equally clear that the definiteness of NPs with possessive specifiers has nothing to do with the semantics of 'possession' (greatly simplifying the meaning of possessives); the suggestion that possession is somehow incompatible with indefiniteness in the thing possessed cannot be right, since there is no anomaly in the phrase *some books belonging to John*. I believe the explanation for the definiteness is syntactic, residing in the position of the possessive expression in the specifier.

Semantically, the way it works is as follows. In *John's books are boring*, the intersection of the set of books belonging to John with the set corresponding to *boring* must be identical with the set of books belonging to John. There are two important points to note here. First, the possessive, though not in N', forms part of the set expression. The only way to avoid this conclusion is to say that the truth value of the sentence depends on the intersection of three sets, the *books* set, the *boring* set and the set of John's possessions; but this would be to give possessives an interpretation very different from that of other 'definite determiners'. Second, the restriction imposed by the presence of the possessive in the specifier is the same as that imposed by *the*. Thus in the absence of a Det such as *most*, which, lexically, specifies the nature of the restriction, it is the restriction corresponding to *the* which holds; *the* is thus in some sense the basic, or unmarked, definite Det. So if English permitted the structure $[_{NP}$*good* $[_{N'}$*men*$]]$, it would have the same interpretation as $[_{NP}$ *the* $[_{N'}$ *good men*$]]$.

In the same way the interpretation of $[_{NP}$ Poss $[_{N'}$ N$]]$ (which we do have in English) is the same as it would be for $[_{NP}$ *the* $[_{N'}$ Poss N$]]$. This latter is precisely the structure we get in some languages, for example Italian *il mio libro*, Spanish *el libro mío* (as well as *mi libro*), where the possessive is in a normal adjective position. It is significant that in Italian possessives can occur with the definite and the 'indefinite' article - *un mio libro* 'a book of mine', so it is clear that the presence of a possessive in pre-N position does not by itself compel a definite interpretation. On my analysis the difference between English and Italian is that in English the possessive is in specifier position while in Italian it is within N' in modifier position; thus in Italian there is nothing to prevent the specifier being filled by the definite article. These two patterns are both widespread (as well as there being languages like Spanish combining both patterns), and I shall for convenience refer to a typological distinction, discussed in Lyons (1985), between DG ('determiner-genitive') and AG ('adjectival genitive') languages, a distinction which may reflect a parameter of phrase structure and lexical insertion. French and German are DG like English, while Portuguese and Greek are AG like Italian; notice that the distinction between genitive Case-marked NPs in languages that have them (English, German, Greek) and agreement-marked possessives seems to have no bearing on the matter. It is important to note too that the question of whether or not a language has a definite article seems not to be relevant. Russian has no definite article, and is DG; *moja kniga* 'my book' can only be understood as definite. On the other hand, according to Huang (1984), Chinese, which also lacks a definite article, is AG; the corresponding phrase can be understood as 'my book' or 'a book of mine'. In AG languages, as noted, the same construction is used for both definite and indefinite, but in DG languages, since a possessive specifier forces a definite interpretation, a different construction has to be used to get the indefinite sense, and the typical pattern is to use a PP complement: French *un livre à moi*, German *ein Buch von mir*, English *a book of mine* (the structure of which is discussed in Lyons 1986). This PP construction has the effect of placing the possessive expression within N'.

The central point of this discussion is that there is nothing in the meaning of possessives to induce definiteness in the NP, and that for this reason a possessive occurring in modifier position does not do so; it simply behaves like any adjectival element, contributing to the set expression. Only when a possessive occurs in the specifier must the NP be interpreted as definite; in this case, the possessive phrase has the same effect as would the Det *the* in this position, while still contributing its lexical content to the set expression in N'.

This treatment has interesting consequences for (in)definiteness generally. First, since the mere filling of the specifier induces definiteness, it follows that indefinite NPs differ syntactically from definites in having empty specifiers, a claim which finds support from the many languages in which there is no 'indefinite article' and absence of a determiner marks an NP as indefinite (for example Chamorro, Irish, Classical Greek). Whether we can go further and say that indefinites have no specifier, because no N" node, thus being non-maximal projections, is a question that I will not pursue here; but such a view would be close to the proposals of Rothstein and Reed (1985), arising from other considerations. Second, as observed above, the contribution to the NP interpretation made by the definite article *the* is a 'default' value for definite NPs - an expression which is not lexically definite can make the same contribution, by simply occupying specifier position; see also Higginbotham (1983: 416). Barwise and Cooper (1981) do not give an analysis of demonstratives, but say that these should turn out to be definite, a view with intuitive appeal and generally assumed to be correct. But if their definiteness consists of the same constraint as holds with *the*, and this constraint is a default value, applying when anything which is not a definite Det appears in specifier position, perhaps they need not be lexically specified as definite. Let us suppose that they are not, but that 'demonstrativeness', by contrast with 'possession', is semantically incompatible with indefiniteness, so that an indefinite NP containing a demonstrative expression would be anomalous; in fact such NPs seem not to occur. Now, it follows from this supposition that any NP containing a demonstrative must have its specifier filled; and indeed, in English and many other languages, the demonstrative itself occupies specifier position. But since, as I am supposing, demonstratives are not lexically definite, it should be possible for them to occur elsewhere in the NP, as long as something appears in the specifier. And in fact in many languages (including Spanish, Arabic), demonstratives either must or can occur in modifier position, but the equivalent of *the* then appears in the specifier (Spanish *este libro* or *el libro este* 'this book').

In conclusion, I have considered one aspect of how syntactic factors can interact with semantic considerations in the grammar of definiteness. I have tried to show on the basis of the possessive facts that the filling of a particular structural position can produce definiteness where the lexico-semantic content of the expression involved is not enough by itself to do this.

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PROCESSING RELATIVE CLAUSES IN BASQUE AND SPANISH*

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Abstract

Two different conceptions about speech processing were examined in two experiments. Experiment 1 used a comprehension task to test native speakers of a VO language (Spanish) learning an OV language (Basque). The results provided strong support for the idea that the configurational properties of sentences play a crucial role in language processing (in particular for the *Interruption Hypothesis*, Slobin, 1971), as opposed to the idea that it is the grammatical relations holding between elements of a sentence that affect the relative difficulty of processing (as claimed by the *Accessibility Hierarchy Hypothesis*, Keenan & Comrie, 1977). Experiment 2 tested native speakers of a VO language (English) learning another VO language (Spanish). Experiment 2, while failing to provide support for either of the hypotheses raised a very interesting theoretical issue: the idea that when parameters have to be reset in L2 learning acquisition is hindered to a considerable extent.

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1. Introduction

The aim of this paper is to determine what factors have an influence on Relative Clause (RC) processing. Two different conceptions about speech processing will be explored (following Hakuta, 1981): one based on the idea that speech is processed according to the *configurational properties* of sentences, and the other based on the idea that speech is processed according to the underlying *grammatical relations* within a sentence. We assume that these ideas about language processing apply universally, irrespective of the choice of parameters of specific languages according to the options that universal grammar makes available (Chomsky 1980, 1981).

Second language learning has often provided adequate grounds for testing hypotheses about speech processing. We are interested here in the relative difficulty of processing a very specific syntactic structure, namely, Relative Clauses (RCs) by native speakers of Spanish (VO language) learning Basque (OV language) and native speakers of English (VO language) learning Spanish. A restrictive relative clause is a noun modifier and forms a syntactic unit with the noun it modifies. Its position to the right or to the left of the head noun within an NP will depend on how a particular language instantiates the *head parameter*. OV languages are normally leftward expanding - heads occur to the right in their phrasal categories and follow their modifiers so that the linear sequence is built from right to left. Conversely, VO languages are normally rightward expanding - heads occur to the left in their phrasal categories and are followed by their modifiers so that the linear sequence is built from left to right.[1] The position of the RC is thus inherently tied to the syntactic organisation of the language.

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[1] Languages are not always consistent in their branching direction. In Basque, genitives, adjectives and RCs behave quite differently within a N",with genitives and RCs to the left of the head noun and adjectives to its right. Eguzkitza (1987: 13-17) argues that genitives and adjectives hang from different levels in the component structure of N" (i), which makes him reach the conclusion that Basque is uniformly head-last as to the projection of N. However, in our opinion, this uniformity becomes obscure when one considers RCs. RCs seem to behave like genitives as to their position to the left or the right of the head noun, but at the same time, they are adjoined

In Basque (1b) the RC precedes its head, and there are no relative pronouns (RPs).[2] Instead, we find an invariable relative marker (RM) whose shape is-*(e)n*.[3] The head noun is the object of the RC and is missing from it. Basque RCs do not involve movement. What we find is a phonologically empty NP that is coindexed with the head noun (Eguzkitza, 1987: 15), so there is no overt marker of the syntactic function of the relativised NP in the RC. The difference between the structure of a RC in English and Spanish, on the one hand, and Basque, on the other, is made explicit in (2).

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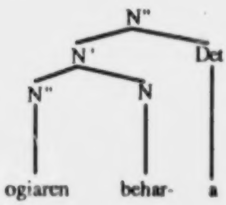
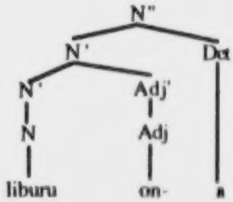
to N', as adjectives are. The behaviour of RCs does thus reveal an inconsistency in the language.

(i) a. ogiaren beharra
bread-det/sg-gen need-det/sg-abs
'the need for bread'

b. liburu ona
book good-det/sg-abs
'the good book'





[2] We are aware of the status of English *that* and Spanish *que* as relative markers. However we are using the the general term 'relative pronoun' with reference to any wh-phrase that introduces a RC in Spanish and English, as opposed to what we find in languages with a consistently invariable relative marker, like Basque.

[3] In the past the form of the RM and that of the past tense morpheme merge so that there is no distinction between the relativised and the non-relativised form of the V. A string like (i) can be interpreted as *the book that the father bought* and *the father bought the book*, Basque being a typical 'scrambling' language superficially, in which almost every possibility of word order is open (Eguzkitza, 1987: 130).

(i) Aitak erosi zuen liburua
father-det/sg-erg to-buy did-it(O)/he(S) book-det/sg-abs

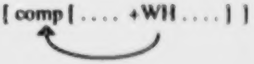
(1) a. He leido [el libro [que han comprado]]
have-I read the book that have-they bought

b. [[Erosi duten] liburua] irakurri
to-buy have-it(O)/they(S) that book-det/abs/sg to-read

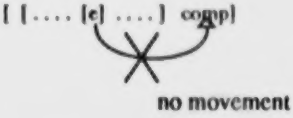
dut
have-it(O)/I(S)

'I have read the book that they have bought'

(2)a. SPANISH/ENGLISH



b. BASQUE



It is worth noting that all major categories show morphological case-marking in Basque (there are at least fifteen cases according to Azkárate et al, 1981: 45). As Eguzkitza (1987: 6) has pointed out Basque relies on 'word shape' as an indicator of grammatical relations, allowing a great variety of possible word orders in s-structure. The head noun of a RC takes the case-ending that corresponds to its function in the main clause so that there is no overt indication of its grammatical role in the RC. The fact that grammatical relations between Subject (S), Direct Object (DO) and Indirect Object (IO) are explicit in the shape of the auxiliary verb helps to minimize the effect of the inflectional loss in the RC. However, S, DO and IO are not the only positions that can be relativised in Basque. Speakers of the main dialect relativise other positions as well, such as directional, ablative, locative and instrumental (De Rijk, 1972: 119), which lose their inflectional markers making RC processing a potentially difficult task in Basque.

2. Strategies for RC processing

Listeners, when hearing a string of words, try to segment this continuum into constituents in order to recover the grammatical relations underlying the different components of a sentence. In parsing a string listeners use certain strategies which are normally based on specific properties of the s-structure of their language and which typically involve word order and the identification of function words and/or inflectional markers. RCs present two main problems for the successful completion of this task (Antinucci et al, 1979):

1. Listeners must identify the elements that belong to the main clause and separate them from the sequence of items that constitute the RC.
2. Listeners must recover the function of the missing NP in the RC.

Language-specific strategies are in turn affected by what seem to be universal operating principles concerning the processing of complex structures in general and RCs in particular. These are language-independent mental operations which are part of the language user's cognitive system and are used in speech processing, where they interact closely with the speaker-hearer's knowledge of the grammatical system of a particular language. There are conflicting opinions in the literature about what (universal) strategies have an effect on the processing of RCs. However, this variety of theories and experimental work can be reduced to a conflict between two conceptions of speech processing (following Hakuta, 1981): theories based on the idea that s-structure *configurational properties* of specific languages are the most important factor affecting RC processing and theories in which processing is intimately related to the *grammatical relations* of the linguistic components of a RC, in particular the NPs involved in relativisation.

Theories based on configurational properties of sentences (mainly, Bever, 1970; Slobin, 1971) are concerned with the way the presence of a RC affects word order and the linear sequence of the elements in a sentence containing a RC. In a pioneering work on language processing, Bever (1970) claimed that, in VO languages, any first NV(N) sequence is assumed by listeners to correspond to a main clause unless the V is clearly marked as subordinate. This strategy was based on the idea that speech is processed sequentially as it is heard. It follows from this assumption that discontinuous constituents, whose elements are not expressed sequentially in surface structure word order, are likely to create difficulties in processing. This is the central idea underlying what we will call the *Interruption Hypothesis* (Slobin, 1971).

Standard English shows a relatively fixed SVO order. It therefore seems reasonable to think that listeners will exploit word order strategies to the maximum in order to parse English input. In Spanish, where word order is more flexible, the relation between S and V is made explicit in the verb ending. As for Basque, it shows a dominant SOV order, although practically all permutations of major categories are possible in declarative sentences provided that the position immediately preceding the main V is filled. This is the focus position or *galdegaia* (*galde* 'ask', *gaia* 'subject' - the subject one asks about).[4] Table 1 shows how the presence of a RC in S and O position can alter the linear sequence of the dominant order in Spanish and English, on the one hand, and Basque, on the other. According to the Interruption Hypothesis, centre-embedded RCs, which 'interrupt' the main clause by positioning themselves immediately after S in English and Spanish (SS, SO) and after O in Basque (OS, OO), are more difficult to process than right-/left-embedded RCs owing to constraints on short-term memory (Slobin, 1971: 42).[5]

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[4] For a full description of the *galdegaia* position see Eguzkitza (1987: 87-121).

[5] According to Kuno (1974) languages will use devices to minimize the effect of those structures that create processing difficulty, such as centre-embedding RCs. Examples of such devices are the choice between prenominal and postnominal RCs and clause-initial and clause-final markers. By looking at Table 1, we can see that postnominal positioning of RCs in Basque would create centre-embedding with both subject and object RCs.

TABLE 1

Sentence type	SPANISH/ENGLISH	BASQUE
SS	N(S)[RP V N(O)] V N(O)	[N(O) V RM] N(S) N(O) V
SO	N(S)[RP N(S) V] V N(O)	[N(S) V RM] N(S) N(O) V
OS	N(S) V N(O)[RP V N(O)]	N(S) [N(O) V RM] N(O) V
OO	N(S) V N(O)[RP N(S) V]	N(S) [N(S) V RM] N(O) V

As for Bever's NV(N) strategy, we can see that in Spanish and English the SVO sequence of the main clause remains unaltered in sentences of the type OS and OO, where O is the head noun of the RC, so we would expect listeners to interpret the initial SVO sequence correctly as a main clause. When the head noun is S the presence of an obligatory relative pronoun (RP) in Spanish stops any main clause interpretation of the initial sequences of SS and SO. In Standard English, where the relative pronoun is optional in SO and OO, the presence of an initial NNV sequence in SO structures blocks any incorrect segmentation of the sentence.

In Basque the first N(N)V sequence in the four sentence types is an initial candidate as a main clause until the relative marker (RM) is heard, since neither S nor O need be lexically present. One could argue, however, that in practice, S and O are only likely to be deleted when they are *given information*, and from a semantic/pragmatic perspective, speakers use RCs when they want to provide the listener with some crucial information concerning a particular object or set of objects, the head noun, which is precisely the element missing in the RC. It is thus unlikely that listeners are expecting a sentence with a missing S or O in such contexts, so that sentences of the type SS and SO should not constitute a problem from a semantic/pragmatic point of view. Since in OO sentences the presence of two NPs carrying ergative markers stops any main clause interpretation the only sentence type that is likely to create confusion is OS.[6] By the time the perceiver hears the suffix of the relative marker: *-en* (which in the past merges with the past tense morpheme: *-en*) in (3b) s/he has already heard an N(S) an N(O) and a V with the ending for the present habitual *-tzen*. The listener is now expecting a main verb auxiliary to complete the sentence. The presence of the relative marker tells the listener to look ahead for the coming NP *erizaina* 'the nurse' and to work out its function within the RC while s/he is listening for the end of the sentence.[7]

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[6] Even in the case of OO the homophony between the ergative and the plural marker /-k/ could lead to confusion in Basque, until the form of the

(3) a. Pazienteak medikua gorrotatzen
patient doctor hate

duen
does-det/erg/sg-det/abs/sg pres.habitual-he(S)/he(O)-RM

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auxiliary V is heard (i). Thus, the presence of /-it-/ in *zituen* indicates that the O of the transitive V is plural. The absence of /-it-/ in (ii) blocks any main clause interpretation of the first NNV sequence.

(i) Gizonak neskak ikusi zituen.
man-det/sg-erg girl-det/pl-abs to-see did-them(O)-he(O)
'The man saw the girls'

(ii) Gizonak [neskak ikusi zuen] mutila
-det/sg-erg did-he(O)-she(S) boy-det/sg-abs

jo zuen
to-hit did-he(O)-he(S)

'The man hit the boy that the girl saw'

[7] Nothing has been said about intonation. According to Clancy *et al* (1986) the role of intonation appears to be significant only when sentences cannot be processed in a simple left-to-right fashion. Their results in an experiment on RC processing by Korean children suggest that there is no significant difference in the understanding of sentences pronounced with clear or monotone intonation.

erizaina maite du
nurse-det/abs/sg to-love does-he(S)/he(O)

'The patient loves the nurse who hates the doctor'

b. Pazienteak medikua gorrotatzen du.
'The patient hates the doctor'

In English and Spanish sentences containing RCs both word order and the presence of relative pronouns clearly mark the V as subordinate, thus making the segmentation of speech a relatively easy task. As for Basque, we have already said that the initial sequences in sentences containing subject and object RCs are potential problems for correct parsing. Therefore, it is only in an OV language like Basque that we will consider the effect of Bever's initial sequence strategy on the processing of RCs. We must then 'rename' it the *NNV Strategy*.

Hypotheses concerning the rearrangement of the linear sequence of linguistic units have received different names in the literature (Sheldon, 1974, 1976; Prideaux, 1982). What we will call with Sheldon the *Word Order Hypothesis* claims that when word order is preserved in the RC the sentence is easier to process. Thus, English and Spanish SS and OS, in which the relativised NP is the S of the RC (subject focus), are predicted to be easier to process than sentences with object focus (SO, OO), since the canonical order is preserved in the RC with the RP occupying the position of the missing S. However, it is difficult to determine what this strategy predicts for Basque where both N(S)V and N(O)V are possible candidates for canonical order.[8]

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[8] Prideaux (1982: 26) in his analysis of Japanese RCs argues that N(O)V structures are more 'canonical' than N(S)V structures in SOV languages like Japanese, since the S is normally given information, and thus, it is more likely to be omitted. This renders N(O)V as the structure preserving the canonical word order. In Basque the existence of the *galdegaia* principle - the focus position - makes it difficult to reach a conclusion about whether SV or OV preserve the canonical word order.

The predictions made by the Word Order Hypothesis match the predictions of what we will call the *Accessibility Hierarchy Hypothesis* (Keenan & Comrie, 1977), which belongs to the second set of hypotheses mentioned above - those concerned with the grammatical relations between NPs in a sentence with a RC. The *accessibility hierarchy* in (4) is claimed to be a universal expression of the relative accessibility to relativisation of NP positions (Keenan & Comrie, 1977: 66) - the lower the position in the hierarchy, the harder it is to process the sentence. Consequently, sentences containing RCs with subject focus (SS, OS) ought to be easier to process than those with object focus (SO, OO) in ALL languages.

(4) SU > DO > IO > OBL > GEN > O COMP

English is classified as a language that allows relativisation in all positions in the hierarchy, and Spanish as a language that allows relativisation in all positions but O COMP (Keenan & Comrie, 1977: 74) (for criticism of this classification of Spanish, see Alcoba, 1985: 102). Basque allows relativisation in only S, DO, and IO positions according to Keenan & Comrie (1977: 72). They also suggest that relativisation in any other position in the hierarchy would create processing difficulties, but not in these three positions since the relation between S(erg), DO(abs) and IO(dat) is explicit in the form of the auxiliary V. However, sentence (5) is an example of ambiguity where the head noun can be interpreted as the IO of the RC (a) or the S of the RC (b). Difficulties are even greater when other positions are relativised (we refer the reader back to the introduction for a description of Basque RCs).

(5) liburua eman di-o-n gizona
book-det/sg-abs to-give have-it-to-him that man-det/sg-abs

nire aita da
my father-det/sg-abs is

a. 'The man that he has given the book to is my father'
b. 'The man that has given him the book is my father'

The relevant factor underlying the idea of language processing in the Accessibility Hierarchy is that perceptual difficulty follows from the grammatical role of the relativised NP in the RC. Processing a sentence with a relativised O is a more difficult task than processing a sentence with a relativised S. Another idea connected with the grammatical role of NPs is presented in Sheldon's (1974) *Parallel Function Hypothesis*. She claims that sentences in which the relativised NP and the head noun have the same grammatical role in their respective clauses (SS, OO) are easier to process than sentences in which the role of the relativised NP in the RC and that of the head noun in the main clause are different (SO, OS). Table 2 provides a summary of the predictions made by the different hypotheses.

TABLE 2

	SOV LANGUAGES				SVO LANGUAGES			
	SS	SO	OS	OO	SS	SO	OS	OO
IH	+	+	-	-	-	-	+	+
WOH	(+)	(-)	(+)	(-)	+	-	+	-
NNV S			-					
AH	+	-	+	-	+	-	+	-
PFH	+	-	-	+	+	-	-	+

(**IH**: Interruption Hypothesis; **WOH**: Word Order Hypothesis; **NNV S**: NNV strategy; **AH**: Accessibility Hierarchy Hypothesis; **PFH**: Parallel Function Hypothesis; + : easy to process; - : difficult to process)

This theoretical controversy is as yet incompletely resolved by the empirical work supporting the different hypotheses. Slobin's (1971) *Interruption Hypothesis* and the idea that centre-embedding causes problems in RC comprehension (Kuno, 1974) have received strong support from experiments involving L2 learners and children (Hakuta,1981; Clancy *et al*, 1986). These experiments provide strong evidence for the effect of the Interruption Hypothesis on RC comprehension in languages with a dominant SOV order. However, the results obtained for SVO languages, like English, have proved 'disappointingly inconsistent' (Clancy *et al*, 1986: 252). There is also a large number of experiments providing evidence for the *Accessibility Hierarchy Hypothesis* for both SOV and SVO languages (Sheldon, 1976; Prideaux, 1982 ; De Villiers *et al*, 1979; Cook, 1975). As for the *Parallel Function Hypothesis* the only evidence we have found is in Sheldon (1974).

3. The Experiments

The following experiments were designed to test the comprehension of subject and object RCs in an attempt to determine the relative difficulty of processing different types of RCs. Experiment 1 tested the comprehension of Basque subject and object RCs by native speakers of Spanish learning Basque. The results obtained provided strong evidence to support the *Interruption Hypothesis*. Experiment 2 was an attempt to extend these results to SVO languages such as English and Spanish. The results obtained failed to support any of the hypotheses tested. This was explained in terms of the interaction between L2 acquisition and theories of universal grammar.

Two competing hypotheses were tested:

HYPOTHESIS 1: people rely on the *configurational properties* of sentences in RC processing - it is the location of the RC that accounts for the relative complexity of processing. In a comprehension test we would expect more errors in sentences with centre-embedded RCs than in those with right-/left-embedded RCs (see Table 3).

HYPOTHESIS 2: people rely on the *grammatical roles* of the NPs involved in relativisation in RC processing. It is the factor of which NP is relativised that accounts for the relative complexity of processing. In a comprehension test we would expect more errors in sentences with relativised N(O) than in those with relativised N(S) (see Table 3).

These hypotheses are the result of the combination of two variables, following Sheldon (1976):

1. The effect of the location of the RC in the sentence: that is whether the RC is right-/left-embedded or centre-embedded.
2. The factor of which NP is relativised: that is, whether the relativised NP is the S or the O of the RC.

Variable 1 has an effect on the configurational properties of the main sentence: the linear order of constituents in s-structure. Of the three hypotheses mentioned in relation to this - Interruption Hypothesis, Word Order Hypothesis and NNV Strategy - (see Table 2), we decided to test only one, the *Interruption Hypothesis* (IH), for simplicity. The Interruption Hypothesis predicts that centre-embedded RCs are more difficult to process than right-/left-embedded RCs. Variable 2 concerns the grammatical roles of the NPs involved in relativisation. Both the Accessibility Hierarchy Hypothesis and the Parallel Function Hypothesis relate to this (see Table 2). We tested only the *Accessibility Hierarchy Hypothesis* (AH), which predicts that sentences in which the relativised NP is the O of the RC are more difficult to process than those in which the relativised NP is the S of the RC, all other things being equal. By combining these two variables we have four types of sentence, examples of which in the languages involved are shown in Table 4.

BASQUE	SPANISH	ENGLISH
gizon-a-k	*el hombre*	
man-det/sg-erg	det/sg-masc man	'the man'
mutil-a	*a-l chico*	
boy-det/sg-abs	to-det/sg-masc boy	'the boy'
neska	*a la chica*	
girl-det/sg-abs	to det/sg-fem girl	'the girl'
neska-k	*la chica*	
girl-det/sg-erg	det/sg-fem girl	'the girl'
jo zuen	*golpeó*	
to-hit did-3rd.p(O) 3rd.p(S)	did hit-3rd.p(S)	'hit'
ikusi zuen	*vio*	
to-see did-3rd.p(O)	did see-3rd.p(S)	'saw'
-en (in *zuen*)	*que*	'that'

TABLE 3
Predictions for the processing of RCs

	BASQUE				SPANISH/ENGLISH			
	SS	SO	OS	OO	SS	SO	OS	OO
IH	easy	easy	hard	hard	hard	hard	easy	easy
AH	easy	hard	easy	hard	easy	hard	easy	hard

TABLE 4
Sentence types

SS: MATRIX NP = SUBJECT RELATIVISED NP = SUBJECT

Neska ikusi zuen gizonak mutila jo zuen.
El hombre *que vio a la chica* golpeó al chico.
The man *that saw the girl* hit the boy.

SO: MATRIX NP = SUBJECT RELATIVISED NP = OBJECT

Neskak ikusi zuen gizonak mutila jo zuen.
El hombre *al que la chica vio* golpeó al chico.
The man *(that) the girl saw* hit the boy.

OS: MATRIX NP = OBJECT RELATIVISED NP = SUBJECT

Gizonak *neska ikusi zuen* mutila jo zuen.
El hombre golpeó al chico *que vio a la chica.*
The man hit the boy *that saw the girl.*

OO: MATRIX NP = OBJECT RELATIVISED NP = OBJECT

Gizonak *neskak ikusi zuen* mutila jo zuen.
El hombre golpeó al chico *al que la chica vio.*
The man hit the boy *(that) the girl saw.*

3.1 Experiment 1

Subjects:

42 Spanish-speaking adults learning Basque. Their average age was 25. They were attending intensive courses in Basque - 4 hours a day - at two different *euskaltegi* (Basque language schools subsidised by the Basque Government). They were at a very advanced level in their learning of the language; some were doing their 7th or 8th three-month course (out of 8) and others were doing a special course in preparation for the official certificate of proficiency in Basque, called *E.G.A.*, issued by the Basque Government. Ss were told they were doing a psycholinguistic experiment and the aim of the experiment was explained to them in general terms.

Materials and Procedure:

Ss were given four sheets of paper with the possible answers for a comprehension test and a separate answer sheet where they had to indicate (by writing a single letter *a*, *b* or *c*) which they thought was correct as well as the degree of confidence in their choice on a 5-point scale. The experiment was done in two different *euskaltegi* in four sessions in total. Ss were divided into two groups in each *euskaltegi* and listened to 20 sentences in Basque, which had been pseudo-randomised and which were presented in a different order to each group. There were 5 sets of sentences each consisting of the 4 types of sentence in Table 4. Sentences were read to the subjects in one of the *euskaltegi* and were played on a tape in the other.

On the sheets given to the subjects there were three paraphrases in Spanish for each of the sentences they had just heard in Basque. The paraphrases consisted of conjoined sentences constructed according to the following pattern, which was, of course, presented in several different orders for the different sentences: a) none of the conjoined sentences corresponded to the meaning of either the RC or the main clause in the Basque sentence, b) only one of the conjoined sentences corresponded to the meaning of either the RC or the main clause in the Basque sentence and c) both conjoined sentences corresponded to the meaning expressed in the Basque sentence (6) (see Table 3 - key to translation).

(6) Neska ikusi zuen gizonak mutila jo zuen
'The man that saw the girl hit the boy'

a. La chica vio al hombre y el chico golpeó al hombre.
'The girl saw the man and the boy hit the man'

b. El hombre vio a la chica y el chico golpeó al hombre.
'The man saw the girl and the boy hit the man'

c. El hombre vio a la chica y el hombre golpeó al chico.
'The man saw the girl and the man hit the boy'

Two different procedures were used:

Method A - Ss listened to the 20 sentences once, with a minute's interval between each of them. During that time (perhaps unnecessarily long) they had to decide whether the correct answer was a, b, or c and rate their confidence in their choice.

Method B was exactly like Method A with the difference that sentences were heard twice – the second time being 15 seconds after the first time, but still with a minute's interval between successive different sentences.

Instructions were given in Basque, illustrated with an example to make sure Ss understood what they were expected to do.The experiment was conducted in a classroom situation by the teachers of the four groups that took part in it. It was left to the teacher of each group to decide which method to use as well as whether to read the sentences or have them on tape. 23 Ss were tested under Method A and 19 under Method B.

Results and Discussion:

Table 5 shows the mean number of correct responses to the 20 Basque sentences. For each subject there were five possible correct answers for each sentence type. Performance increased slightly for Method B when each sentence was heard twice. To test the *Accessibility Hierarchy Hypothesis* (AH) we compared the scores obtained for sentences of the type SS with the scores for SO, and OS with OO using a paired t-test (two-tailed). Conversely, to test the *Interruption Hypothesis* (IH), we compared SS with OS, and SO with OO (see Table 6).

TABLE 5
Mean number of correct responses for the four sentence types

	SS	SO	OS	OO
METHOD A	3.2	3.3	1.6	2.5
METHOD B	3.3	4.0	2.2	2.5

TABLE 6
Analysis of mean substractions using a paired t-test

		AH		IH	
		SS-SO	OS-OO	SS-OS	SO-OO
METHOD A	MEAN	-0.17	-0.87	1.56	0.87
	P VALUE	0.56	0.01	0.00	0.02
METHOD B	MEAN	-0.63	-0.31	1.10	1.42
	P VALUE	0.09	0.37	0.01	0.00

Our experiment failed to provide support for the *Accessibility Hierarchy Hypothesis*. Performance on SS is lower than performance on SO (though not to a significant extent) and also contrary to what the Accessibility Hierarchy Hypothesis predicts there were more correct responses for OO than for OS, the difference being highly significant for Method A. On the other hand, SS scores are significantly higher than OS scores and SO are significantly higher than OO, providing strong evidence for the predictions made by the *Interruption Hypothesis*, which claims that centre-embedded RCs create difficulty in processing.

The confidence scores (Tables 7 and 8) tend to confirm the hypothesis that centre-embedded RCs create perceptual confusion. The order of the mean confidence rates matches the scores obtained in the comprehension task. We used a non-parametric test (Wilcoxon test) to compare the paired confidence scores of all Ss in Experiment 1 for the four sentence types. The analysis provided further support for the Interruption Hypothesis - the mean difference between the pairs SS and OS and SO and OO being statistically significant. As for the Accessibility Hierarchy Hypothesis, SS-SO fails even to approach significance and although OS-OO does reach significance, as with the results for the comprehension task, the difference lies in the opposite direction from the predictions made by the Accessibility Hierarchy.

TABLE 7
Mean confidence rates

SS	SO	OS	OO
3.3	3.4	2.7	3.0

TABLE 8
Analysis of confidence rates using Wilcoxon test

	SS-SO	OS-OO	SS-OS	SO-OO
P VALUE	0.5	0.02	0	0

General discussion:

The results for the comprehension task and the confidence scores in Experiment 1 provide strong support for the Interruption Hypothesis. When the linear order of constituents is 'interrupted' in surface structure, comprehensibility is lower owing to the load on short-term memory. It is also worth noting that OS was the sentence type that had the lowest mean in both the comprehension test and the confidence scores, which is in accordance with the predictions made by the NNV strategy (see Table 2).[9] Thus, the results in Experiment 1 suggest that it is the configurational properties of sentences that account for the difficulty of RC processing in Basque. The experiment failed to support the predictions of the Accessibility Hierarchy Hypothesis. In this section we will examine what we consider to be some basic problems underlying this hypothesis: in particular, the lack of theoretical and empirical evidence supporting the psychological validity of the hypothesis and the basic confusion between processing mechanisms and grammatical description.

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[9] It seems that despite intonational clues subjects still interpreted the first part of sentences like (i) as constituting a main clause:

The *Accessibility Hierarchy Hypothesis* (Keenan & Comrie, 1977) was formulated following a cross-linguistic study of what positions languages can and cannot relativise. There seems to be a general agreement about the descriptive power of the Accessibility Hierarchy Hypothesis across languages (but for criticism see Alcoba, 1985; Manaster-Romer, 1979). It is also predictive in the sense that it claims that if a language can relativise IOs, then it can also relativise DOs and SUs (for criticism see Cinque, 1981). Keenan & Comrie (1977) classify languages according to the positions they can relativise and so, English is classified as a language that can relativise all positions in the hierarchy, Spanish can relativise all positions but O COMP, and Basque can only relativise SU, DO and IO. Our objection is: does this inevitably entail that WITHIN A GIVEN LANGUAGE the positions that can be relativised should be ordered hierarchically, and if so, what does it mean to say that SUs are 'more accessible' to relativisation and so on?

It could be argued that evidence for the existence of the hierarchy within a language can be found in the fact that there are normally more RCs with subject focus than RCs with object focus, an idea present in Keenan (1975: 140). However, this could be explained in terms of discourse constraints. From a discourse point of view a RC makes a statement about a head noun, which can be regarded as the 'theme' of the RC. As it is normally Ss that are 'themes' in sentences, we would ex-

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(i)	Gizonak	neska	ikusi zuen (mutila jo zuen)
	man-det/sg-erg	girl-det/sg-abs	to see did-he(O)-she(S)

'The man saw the girl'

pect to find more relativised NPs in S position in the RC than in any position other than S. The hierarchy could then be justified on discourse grounds, but not on the grammatical properties of RCs.[10] The existence of what we could call a 'discourse hierarchy' could explain the results obtained by Prideaux (1982) and Sheldon (1976) in support of the Accessibility Hierarchy Hypothesis. Both these experiments were based on the *intuitions* of native speakers about the relative complexity of subject and object RCs in their own language. Interestingly, some native speakers of Basque we asked agreed that sentences with object focus were more difficult to understand than sentences with subject focus, which suggests that native speakers' intuitions may well be based on discourse considerations, rather than structural factors.

Even if we accept the existence of the hierarchy on theoretical grounds, its psychological validity remains debatable. There are different interpretations in the literature about what it means to say that Ss are 'more accessible' to relativisation than Os from a psycholinguistic point of view (Gass, 1977: 339; Clancy *et al*, 1986: 229). Our own interpretation of the predictions made by this hypothesis has been that RCs with object focus should be more difficult to understand than RCs with subject focus. However, this is a dangerous statement since it reveals a basic confusion between processing strategies and grammatical description. Also, the Accessibility Hierarchy Hypothesis explains processing difficulty by concentrating on the grammatical role of the relativised NP in the RC, but ignores the role the head noun plays in the main clause. It ignores the fact that RCs are embedded in main clauses and cannot be understood without them. In fact, any hypothesis stated in purely grammatical terms is challenged by the results in Experiment 1, since there are no grounds for attributing any real explanatory power to the *Parallel Function Hypothesis* either: performance on SS and SO was very similar and the difference between OS and OO can be explained more consistently in terms of the NNV strategy.

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[10] In fact Keenan & Comrie (1977: 94) admit that heads of RCs share a logical property with subjects of sentences. Also, Kroch and Hindley (1982, cited in Clancy, 1986: 229) make the observation that even in languages that allow relativisation in all positions Ss are more often relativised than Os and so on. This does not seem to depend on any grammatical property of the sentence, but rather on thematic constraints.

The conclusion to be drawn from these observations is that unless the Accessibility Hierarchy Hypothesis and the Parallel Function Hypothesis are re-stated in such a way that they take into account the role played by the configurational properties of specific languages in RC processing there are strong objections against attributing to them a significant part in RC processing. In this regard it is interesting that the predictions made by the Accessibility Hierarchy Hypothesis coincide with the predictions made by the *Word Order Hypothesis* (see Table 2) and experimental results that have often been mentioned as providing support for the Accessibility Hierarchy Hypothesis (Cook, 1975) could in fact be better explained in terms of the Word Order Hypothesis. Cook himself admits that the reason why sentences with object focus create difficulty is that they disrupt the canonical word order of English (Cook, 1975: 204)

The two hypotheses that receive support from the results in Experiment 1 concentrate on s-structure properties mainly, but they also take into account the grammatical description of a sentence containing a RC. In languages that show a rigid word order, such as English, the grammatical function of the head noun in the main clause affects configurational properties in a crucial way, as the position of the RC in the sentence will be determined by the grammatical role of the head noun. In languages like Basque, in which almost every permutation of elements is possible, any approach to language processing that concentrates on s-structure provides an insight into the grammatical description of the language, since, as Eguzkitza points out (1986: 143), 'in Basque there is no apparent change from the deep structure relations to the surface structure cases'.

In summary, the results obtained for Experiment 1 suggest that, at least for an SOV language like Basque, non-native speakers process RCs attending primarily to s-structure properties of sentences containing RC. The observations derived from the analysis suggest the adequacy of an integrated approach to language processing. In this sense, an account of how RCs are processed in Basque should concentrate on both the universal properties of grammatical description, and configurational properties specific to Basque.

3.2 Experiment 2

Subjects:

19 first and second year students at the University of Leeds (England) reading Spanish as a main subject of their degree. Their command of Spanish was thought to be equivalent to the command of Basque of Ss in Experiment 1. They were all volunteers and were tested in four sessions outside lecture hours. The aim of the experiment was explained to them in general terms.

Materials and Procedure:

The material matched that of Experiment 1. The Basque sentences were translated into Spanish and the questionnaire into English. It is worth noting that in Spanish the preposition *a* before a personal N(O) indicates the accusative case in a sentence in which there are no other NPs in the VP. Although this provides a very important s-structure clue, we decided to use this structure instead of the alternative construction without the preposition, also possible in Spanish, as we considered that the construction with the preposition was more 'natural'. The procedure was exactly the same as for Experiment 1. We used Method A and the sentences were on tape.

(7) El espía abrazó a la chica a la que el detective hirió.
the spy embraced-he to the girl to the that the detective hurt

'The spy embraced the girl (that) the detective hurt.

a. The spy embraced the girl and the detective hurt the girl.
b. The spy embraced the girl and the girl hurt the detective.
c. The spy embraced the detective and the girl hurt the detective.

Results and Discussion:

The slight difference between the mean scores for the four sentence types (Table 9) can be attributed to chance alone. The high scores suggest that the task proved too easy for the Ss, an idea that the confidence scores (Tables 10 and 11) seem to confirm. As in Experiment 1 we compared the difference between the mean confidence scores for the four sentence types using a non-parametric test (Table 12). Ss were more confident about their choices in sentences with subject focus, than in those with object focus as the *Accessibility Hierarchy Hypothesis* predicts. At the same time, no evidence was found that Ss felt more insecure about their choices in sentences with centre-embedded RCs. On the contrary, the difference between SS and OS, on the one hand, and SO and OO, on the other, lies in the opposite direction from what the Interruption Hypothesis predicts.

TABLE 9

Mean number of correct responses

SS	SO	OS	OO
4.6	4.6	4.7	4.5

TABLE 10

Mean confidence rates for the four sentence types

SS	SO	OS	OO
4.3	3.7	4.5	4.1

TABLE 11

Analysis of mean confidence rates in Experiments 1 and 2 by means of Mann-Whitney test

	SS1-SS2	SO1-SO2	OS1-OS2	OO1-OO2
P VALUE	0.00	0.01	0.00	0.00

TABLE 12

Analysis of mean confidence rates in Experiment 2 by means of Wilcoxon test

	SS-SO	OS-OO	SS-OS	SO-OO
P VALUE	0.00	0.00	0.05	0.00

It is difficult to reach a conclusion from these results, since confidence scores are influenced by factors which might not depend upon the incorrect or correct comprehension of a sentence. The reason why we included the confidence task in the experiments was to make sure Ss answered all questions in the comprehension test, even if they did not know the answer. In Experiment 1, the confidence scores provided further support for the results obtained in the comprehension task. In Experiment 2, where the comprehension task proved inadequate, we are left with only the evidence of the confidence scores in support of the Accessibility Hierarchy Hypothesis. In our opinion, no conclusion can be reached until an appropiate comprehension task is designed that can provide results capable of distinguishing between the hypotheses.

Comparison of the comprehension task scores in Experiments 1 and 2 raises an interesting theoretical issue in relation to theories of L2 acquisition and the concepts of parametric variation and markedness in Universal Grammar. In particular, it has been suggested that when L1 and L2 do not match in branching direction (Spanish and Basque, for example) acquisition of L2 will be hindered as the parameters will need to be reset for L2 (White, 1987; Phinney, 1987). Gass (1977) tested language transfer in those areas in which languages of the world differ in their relativisation strategies and found evidence that Universal Grammar plays a leading role in assigning a relative order of difficulty in RC processing. The same conclusion was reached by Flynn (1984) about the acquisition of anaphora in L2 learners of Spanish, Japanese and English.

The higher scores obtained in the comprehension task in Experiment 2 compared to Experiment 1 can then be explained in the light of these theories. English and Spanish are languages with a basically consistent right-branching direction, as opposed to Basque that shows a dominant left-branching direction. When the parameters for branching direction match, acquisition of certain syntactic structures, like RCs, is easier than when there is a mismatch in branching direction. Thus, Ss in Experiment 1 found the task much more difficult than Ss in Experiment 2.

4. Conclusion

Processing RCs presents several perceptual difficulties. After examining different language-specific and language-universal principles affecting RC processing (mainly, Bever, 1970; Slobin, 1971; Sheldon, 1974, 1976; Keenan & Comrie, 1977) we have argued that all these contradictory hypotheses result from the contention between two different ideas about language processing in general and RC processing in particular (following Hakuta, 1981): one based on configurational properties of sentences and the other on the grammatical relations of elements in a sentence. Our own experimental results provided strong support for the idea that it is surface structure properties of sentences that affect processing difficulty. In particular, the *Interruption Hypothesis* (Slobin, 1971) received strong support in Experiment 1 against the *Accessibility Hierarchy Hypothesis* (Keenan & Comrie, 1977), which explains perceptual difficulty on grammatical grounds. It was also suggested that only an integrated approach to language processing could account for the relative difficulty of processing RCs in Basque. The comparison between the results obtained in Experiment 1 and those obtained for the processing of Spanish RCs by English Ss (Experiment 2) raised the issue of the interaction between theories of L2 acquisition and Universal Grammar, particularly the concept of branching direction as an instance of parametric variation. However, we could not draw any conclusion about the relative difficulty of RC processing in an VO language, like Spanish. This question is still open for further research.

APPENDIX: SENTENCES USED IN THE EXPERIMENTS

EXPERIMENT 1

GROUP A

1. Gizona neska ikusi zuen mutila jo zuen.
2. Manifestariak zapaldu zuen poliziak ikaslea atxilotu zuen.
3. Medikua gorrotatzen zuen pazienteak erizaina maite zuen.
4. Espiak detektibea zauritu zuen neska besarkatu zuen.

5. Arbitroa bultzatu zuen jokalariak aurkako taldekoa jo zuen.
6. Pazienteak medikuak gorrotatzen zuen erizaina maite zuen.
7. Neska ikusi zuen gizonak mutila jo zuen.
8. Detektibeak zauritu zuen espiak neska besarkatu zuen.
9. Manifestaria zapaldu zuen poliziak ikaslea atxilotu zuen.
10. Jokalariak arbitroa bultzatu zuen aurkako taldekoa jo zuen.
11. Poliziak manifestariak zapaldu zuen ikaslea atxilotu zuen.
12. Detektibea zauritu zuen espiak neska besarkatu zuen.
13. Arbitroak bultzatu zuen jokalariak aurkako taldekoa jo zuen.
14. Gizonak neska ikusi zuen mutila jo zuen.
15. Medikuak gorrotatzen zuen pazienteak erizaina maite zuen.
16. Poliziak manifestaria zapaldu zuen ikaslea atxilotu zuen.
17. Pazienteak medikua gorrotatzen zuen erizaina maite zuen.
18. Espiak detektibeak zauritu zuen neska maite zuen.
19. Neskak ikusi zuen gizonak mutila jo zuen.
20. Jokalariak arbitroak bultzatu zuen aurkako taldekoa jo zuen.

COMPREHENSION TEST: GROUP A

1. a. El hombre golpeó a la chica y la chica vio al chico.
   b. El hombre golpeó a la chica y el chico vio a la chica.
   c. El hombre golpeó al chico y el chico vio a la chica.
2. a. El manifestante pisó al policía y el policía arrestó al manifestante.
   b. El manifestante pisó al estudiante y el policía arrestó al manifestante.
   c. El estudiante pisó al policía y el policía arrestó al manifestante.
3. a. La enfermera odiaba al médico y el paciente amaba a la enfermera.
   b. El médico odiaba al paciente y la enfermera amaba al paciente.
   c. El paciente odiaba al médico y el paciente amaba a la enfermera.
4. a. El espía abrazó a la chica y el detective hirió a la chica.
   b. La chica abrazó al espía y el detective hirió a la chica.
   c. El espía abrazó a la chica y la chica hirió al detective.
5. a. El jugador empujó al árbitro y el jugador pegó al contrario.
   b. El contrario empujó al árbitro y el jugador pegó al contrario.
   c. El jugador empujó al árbitro y el contrario pegó al jugador.
6. a. El paciente odiaba al médico y el paciente amaba a la enfermera.

b. El médico odiaba a la enfermera y el paciente amaba a la enfermera.
c. El médico odiaba al paciente y la enfermera amaba al paciente.
7. a. La chica vio al hombre y el chico golpeó al hombre.
b. El hombre vio a la chica y el chico golpeó al hombre.
c. El hombre vio a la chica y el hombre golpeó al chico.
8. a. El detective hirió al espía y la chica abrazó al espía.
b. El detective hirió al espía y el espía abrazó a la chica.
c. El detective hirió a la chica y el espía abrazó a la chica.
9. a. El estudiante pisó al manifestante y el policía arrestó al manifestante.
b. El estudiante pisó al manifestante y el policía arrestó al estudiante.
c. El policía pisó al manifestante y el policía arrestó al estudiante.
10. a. El jugador pegó al contrario y el árbitro empujó al contrario.
b. El jugador pegó al contrario y el contrario empujó al árbitro.
c. El jugador pegó al árbitro y el jugador empujó al contrario.
11. a. El policía arrestó al manifestante y el estudiante pisó al manifestante.
b. El policía arrestó al estudiante y el estudiante pisó al manifestante.
c. El policía arrestó al estudiante y el manifestante pisó al estudiante.
12. a. El detective hirió a la chica y el espía abrazó a la chica.
b. El espía hirió al detective y el espía abrazó a la chica.
c. El espía hirió al detective y el detective abrazó a la chica.
13. a. El árbitro empujó al contrario y el jugador pegó al contrario.
b. El árbitro empujó al jugador y el jugador pegó al contrario.
c. El árbitro empujó al contrario y el contrario pegó al jugador.
14. a. El hombre golpeó a la chica y el chico vio al hombre.
b. El hombre golpeó al chico y el chico vio a la chica.
c. El hombre golpeó al chico y la chica vio a la chica.
15. a. El paciente odiaba al médico y el paciente amaba a la enfermera.
b. El médico odiaba al paciente y el paciente amaba a la enfermera.
c. El médico odiaba al paciente y el médico amaba a la enfermera.
16. a. El policía arrestó al estudiante y el estudiante pisó al manifestante.

b. El policía arrestó al estudiante y el policía pisó al manifestante.
c. El policía arrestó al manifestante y el policía pisó al estudiante.
17. a. La enfermera amaba al paciente y el paciente odiaba al médico.
b. El paciente amaba a la enfermera y la enfermera odiaba al médico.
c. El paciente amaba a la enfermera y el médico odiaba a la enfermera.
18. a. El espía abrazó a la chica y el detective hirió a la chica.
b. El espía abrazó a la chica y la chica hirió al detective.
c. El espía abrazó al detective y la chica hirió al detective.
19. a. La chica vio al chico y el chico golpeó al hombre.
b. El hombre vio a la chica y la chica golpeó al chico.
c. La chica vio al hombre y el hombre golpeó al chico.
20. a. El jugador pegó al árbitro y el árbitro empujó al contrario.
b. El jugador pegó al contrario y el árbitro empujó al contrario.
c. El jugador pegó al árbitro y el jugador empujó al contrario.

EXPERIMENT 2

GROUP A

1. El hombre golpeó al chico que vio a la chica.
2. El policía al que el manifestante pisó arrestó al estudiante.
3. El paciente que odiaba al médico amaba a la enfermera.
4. El espía abrazó a la chica que hirió al detective.
5. El jugador que empujó al árbitro golpeó al contrario.
6. El paciente amaba a la enfermera a la que el médico odiaba.
7. El hombre que vio a la chica golpeó al chico.
8. El espía al que hirió el detective abrazó a la chica.
9. El policía que pisó al manifestante arrestó al estudiante.
10. El jugador golpeó al contrario que empujó al árbitro.
11. El policía arrestó al estudiante al que el manifestante pisó.
12. El espía que hirió al detective abrazó a la chica.
13. El jugador al que el árbitro empujó golpeó al contrario.
14. El hombre golpeó al chico al que la chica vio.
15. El paciente al que el médico odiaba amaba a la enfermera.
16. El policía arrestó al estudiante que pisó al manifestante.
17. El paciente amaba a la enfermera que odiaba al médico.

18. El espía abrazó a la chica a la que el detective hirió.
19. El hombre al que la chica vio golpeó al chico.
20. El jugador golpeó al contrario al que el árbitro empujó.

COMPREHENSION TEST: GROUP A

1. a. The man hit the girl and the girl saw the boy.
   b. The man hit the girl and the boy saw the girl.
   c. The man hit the boy and the boy saw the girl.
2. a. The demonstrator stepped on the policeman and the policeman arrested the student.
   b. The demonstrator stepped on the student and the policeman arrested the student.
   c. The student stepped on the policeman and the policeman arrested the demonstrator.
3. a. The nurse hated the doctor and the patient loved the nurse.
   b. The doctor hated the patient and the nurse loved the patient.
   c. The patient hated the doctor and the patient loved the nurse.
4. a. The spy embraced the girl and the detective hurt the girl.
   b. The girl embraced the spy and the detective hurt the girl.
   c. The spy embraced the girl and the girl hurt the detective.
5. a. The football player pushed the referee and the football player hit the opponent.
   b. The opponent pushed the referee and the football player hit the opponent.
   c. The football player pushed the referee and the opponent hit the football player.
6. a. The patient hated the doctor and the patient loved the nurse.
   b. The doctor hated the nurse and the patient loved the nurse.
   c. The doctor hated the patient and the nurse loved the patient.
7. a. The girl saw the man and the boy hit the man.
   b. The man saw the girl and the boy hit the man.
   c. The man saw the girl and the man hit the boy.
8. a. The detective hurt the spy and the girl embraced the spy.
   b. The detective hurt the spy and the spy embraced the girl.
   c. The detective hurt the girl and the spy embraced the girl.

9. a. The student stepped on the demonstrator and the policeman arrested the demonstrator.
   b. The student stepped on the demonstrator and the policeman arrested the student.
   c. The policeman stepped on the demonstrator and the policeman arrested the student.

10. a. The football player hit the opponent and the referee pushed the opponent.
   b. The football player hit the opponent and the opponent pushed the referee.
   c. The football player hit the referee and the football player pushed the opponent.

11. a. The policeman arrested the demonstrator and the student stepped on the demonstrator.
   b. The policeman arrested the student and the student stepped on the demonstrator.
   c. The policeman arrested the student and the demonstrator stepped on the student.

12. a. The detective hurt the girl and the spy embraced the girl.
   b. The spy hurt the detective and the spy embraced the girl.
   c. The spy hurt the detective and the detective embraced the girl.

13. a. The referee pushed the opponent and the football player hit the opponent.
   b. The referee pushed the football player and the football player hit the opponent.
   c. The referee pushed the opponent and the opponent hit the football player.

14. a. The man hit the girl and the boy saw the man.
   b. The man hit the boy and the boy saw the girl.
   c. The man hit the boy and the girl saw the boy.

15. a. The patient hated the doctor and the patient loved the nurse.
   b. The doctor hated the patient and the patient loved the nurse.
   c. The doctor hated the patient and the doctor loved the nurse.

16. a. The policeman arrested the student and the student stepped on the demonstrator.
   b. The policeman arrested the student and the policeman stepped on the demonstrator.

c. The policeman arrested the demonstrator and the policeman stepped on the student.

17. a. The nurse loved the patient and the patient hated the doctor.
    b. The patient loved the nurse and the nurse hated the doctor.
    c. The patient loved the nurse and the doctor hated the nurse.
18. a. The spy embraced the girl and the detective hurt the girl.
    b. The spy embraced the girl and the girl hurt the detective.
    c. The spy embraced the detective and the girl hurt the detective.
19. a. The girl saw the boy and the boy hit the man.
    b. The man saw the girl and the girl hit the boy.
    c. The girl saw the man and the man hit the boy.
20. a. The football player hit the referee and the referee pushed the opponent.
    b. The football player hit the opponent and the referee pushed the opponent.
    c. The football player hit the referee and the football player pushed the opponent.

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A PRAGMATIC VIEW OF FRENCH DEIXIS*

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0. Introduction

French exhibits a number of partially cognate deictic terms (the locative adverbs *ici* and *là*, the demonstrative suffixes *-ci* and *-là*, the pronouns *ceci* and *cela*, the presentatives *voici* and *voilà*, and so on). The conventional English translations of these items involve 'this' or 'here' for the first element of each pair, and 'that' or 'there' for the second. The relationship between the members of these various pairs is, however, unclear. Any contrastive analysis of French and English should account for the lack of correspondence between the two-term deictic systems of these languages — a discrepancy highlighted by the fact that French-English dictionaries, down to phrase-book level, are uneasy about glossing *là* as 'there' and *celui-là* as 'that one', and often give 'here' and 'this one' as alternative translations (see Smith (1988) for discussion and examples). However, just as significantly, the relationship between *ci*-terms and *là*-terms is also an internal problem of French. This paper will examine the opposition between *ici* and *là* (and similar oppositions between various partially cognate deictic terms) and try to draw some tentative conclusions regarding their meaning and distribution. It will be concerned only with exophoric uses of these terms, and not with anaphoric, text-internal reference (*celui-ci* in the sense of 'the latter',

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celui-là in the sense of 'the former', for example). To avoid begging questions and misleading the reader, the French deictics under discussion will not be translated in the English glosses of example sentences.

1. Commonly expressed views

The confusion surrounding the precise nature of the relationship between, on the one hand, *ici* and its cognates (henceforth, '*(i)ci*') and, on the other hand, *là* and its cognates (henceforth, '*là*') can be gauged from the comments of both prescriptive grammarians and descriptive linguists, which are frequently contradictory — sometimes to the point of incoherence. A representative selection of views on the matter reveals three main categories of opinion.

1.1. *(I)ci* is an antonym of *là*

The first, and simplest, view is that *(i)ci* is an **antonym** of *là* — the two items are quite simply mutually exclusive. A large number of authorities imply or state explicitly that the two terms are in opposition, and that the opposition is one of proximity vs. remoteness, with *(i)ci* expressing the former and *là* the latter. This point of view is expressed by, amongst others, Brunot (1926: 423), Wagner & Pinchon (1962: §494), Le Bidois & Le Bidois (1967: §1732), Berrendonner (1979: 349), and Grevisse (1980: §2163), as well as by the *Robert* dictionary (Robert 1978), s.v. *là* (t. 4, p. 7).

1.2. *(I)ci* is a synonym of *là*

The second view is diametrically opposed to the first: it is that, synchronically, *(i)ci* is a **synonym** of *là* (or that, diachronically, *(i)ci* has taken over, or is in the process of taking over, the meaning of *là*). The alleged synonymy is often characterized as vulgar, substandard, or colloquial (depending on the point of view of the commentator); the basic message, however, is that *(i)ci* and *là*, at least some of the time, mean the same thing. For Brunot (1926: 423), the distinction between *ici* and *là* has practically disappeared. According to Bauche (1928: 137), *là* is commoner than *ici* in colloquial French, where it serves to indicate proximity as well as remoteness. Frei (1929: 86) claims that the opposition between *(i)ci* and *là* is no longer one of reference (proximity vs. remoteness), but rather one of register (formal ('relevé') vs. colloquial ('populaire')) — his comments presumably apply only to the expression of proximity. Le Bidois & Le Bidois (1967: §1732) observe that *là* frequently assumes the meaning of *(i)ci*, giving the example *Je suis là* in the sense of *Me voici;* whilst Price (1971: 127) notes that

> In spoken French, the fact that *là* is tending to take over the functions of *ici* (e.g., *Je suis là* 'I'm here') has had the effect of obliterating anew the opposition nearness~distance, since *ce train-là*, *celui-là*, can now mean 'this train, this one' as well as 'that train, that one'.

Wartburg & Zumthor (1973: §729) claim that the opposition between *ici* and *là* comes into play only when the two items occur in the same sentence, and that in other circumstances there is a tendency to substitute *là* for *ici* with the meaning of proximity. The same tendency is noted and castigated by the *Robert* dictionary (Robert 1978), s.v. *là* (t. 4, p. 7), whilst Grevisse (1986: §969) regards it simply as a feature of ordinary language ('la langue commune').

1.3. *(I)ci* is a hyponym of *là*

A third and perhaps more subtle view is that, whilst *(i)ci* and *là* are not synonyms, the opposition between them is *inclusive* rather than *exclusive* — in other words, that one is a **hyponym** of the other. Although they do not use this terminology, it is implicit in the comments of several grammarians that they regard *(i)ci* as in essence a hyponym of *là*. Thus, for Wartburg & Zumthor (1973: §730), the localization expressed by *(i)ci* is more definite and more emphatic — the two can have the same spatial reference, but *(i)ci* pinpoints where *là* does not; for Berrendonner (1979: 354, n2), the opposition between *(i)ci* and *là* is not a negative (understand 'exclusive') one; for Togeby (1982: §388) (and a good many other commentators), *là* is the unmarked term and *(i)ci* the marked one; for Judge & Healey (1983: 81) '*là* is far less precise than *(i)ci*':

> The adverbs *là* and *ici* have, however, lost some of their meaning as specific indicators of place; in particular, *là* seems to be used in a number of contexts in which one would expect *ici*, e.g. (an adult speaking to a child):
>
> | | |
> |---|---|
> | viens ici, à côté de moi! | ['come *ici*, beside me!'] |
> | viens là, à côté de moi! | ['come *là*, beside me!'] |
>
> This is probably due to the fact that *là* is far less precise than *ici*.

— and the *Trésor de la langue française*, s.v. *ici* (t. 9, pp. 1058-1060) defines *là* as a synonym of *ici* which fails to convey the notion of proximity associated with the latter. This curious definition, which appears at first sight to be a contradiction in terms, could be interpreted to mean that *là* has the same semantic description as *ici*, less the feature of proximity — in other words, that *ici* is a hyponym of *là*.

2. Specific differences and incompatibilities between *(i)ci* and *là*

It is unfortunate that few commentators of any persuasion discuss the division of labour between *(i)ci* and *là* in any depth. We can, however, put their views to the test. If we can find a situation to which only one of the two terms is appropriate, then we may reject the claim that they are synonyms. It will be sufficient to find a situation in which *(i)ci* but not *là* may be used (a special case of the above condition) in order to refute the claim that *(i)ci* is a hyponym of *là*.

Secondary sources provide few relevant examples. One of the rare examples of a non-trivial *(i)ci/là* distinction quoted in a work of reference is the one to be found in Judge & Healey (1983: 81), for whom

'...there is a greater sense of urgency associated with *ici* '. They contrast *Viens ici que je te donne une paire de claques!* ('Come *ici*, so I can slap your face!') with *Viens là que je te donne un bonbon!* ('Come *là*, so I can give you a sweet!'). In fact, their examples are to some extent misleading — it is quite possible to say *Viens ici que je te donne un bonbon*, as well as *Viens là*...; however, many native informants agree that **Viens là que je te donne une paire de claques* is an extremely unlikely utterance.

Work with informants has also yielded the following examples:

- Je suis ici ≠ Je suis là

Je suis ici is virtually tautologous, a statement with an information content approaching zero — it could be paraphrased as 'I am where I am'. *Je suis là*, on the other hand, gives information to the addressee — it would be used, for instance, as the (normal) answer to the question *Où es-tu?*, or to announce one's arrival.

- ?*Je ne suis pas ici

By the same token, *Je ne suis pas ici* is felt by many speakers to be at worst a contradiction in terms or at best paradoxical — it seems to imply 'I am not where I am'. Informants find it difficult to provide contexts in which this utterance is plausible. The indication of the actual or effective absence of the first person from a proximate point normally involves the use of *là* — compare *Je ne suis pas là demain* (corresponding to 'I'm not here tomorrow') and *Je ne suis là pour personne* (equivalent to 'If anyone asks, I'm not here'). The only sense in which *Je ne suis pas ici* was acceptable to a majority of informants was a metaphorical one implying distraction (compare English *I'm not all here*).

- Ici Pierre Dupont/*Là Pierre Dupont

A specific instance in which *(i)ci* alone may be used and *là* is impossible is in the formula used when answering the telephone.

- Monsieur Chirac n'est pas ici ≠ Monsieur Chirac n'est pas là

One informant provided an example of an *(i)ci/là* distinction involving the right-wing politician, Monsieur Jacques Chirac. *Monsieur Chirac n'est pas ici* is the response one might get if one telephoned the headquarters of the French Socialist Party and asked for him; *Monsieur Chirac n'est pas là* would not be an appropriate reply in this context, but would be used to inform someone who telephoned him at his own office that he was away or otherwise unavailable. Compare also *Il n'est pas ici /*là; son bureau est en face* ('He isn't *ici /*là*; his office is across the corridor' — i.e., 'You've got the wrong office') and *Il n'est pas là/*ici; est-ce que vous pourriez repasser?* ('He isn't *là/*ici*; could you come back later? — i.e., 'You've got the right office, but...'). *Il n'est pas ici* denotes **essential** absence, whilst *Il n'est pas là* denotes **contingent** absence. Although it might be possible to force this distinction into the 'hyponym' mould, the use of *ici* in such cases is not really a 'proximate' one; it more accurately indicates corporate identity ('He doesn't work here' or 'He doesn't work for us').

- Viens un peu ici /??Viens un peu là

Finally, we return to the use of *ici* and *là* with the imperative *viens*. Whilst *Viens là* and *Viens ici* are perceived as equally acceptable and approximately synonymous, a majority of informants reject *là* in the presence of the intensifying adverbial *un peu*; thus, *Viens un peu ici* (in the approximate sense of 'Just you come here') is acceptable, but **Viens un peu là* is not. The distinction ties in with that noted by Judge & Healey, and also with the fact that, for several informants, *Viens ici* is the only one of the two utterances which can carry the intonation pattern associated with impatience. One informant pointed out that, in a context of increasing exasperation, the sequence *Viens là, viens là... mais viens ici!* was normal, whereas the sequence *Viens ici, viens ici... mais viens là!* would be bizarre.

3. The rôle of *là-bas*

If we now turn to the expression *là-bas*, the original meaning of which was 'down there' or 'there below' (the only glosses given by Cotgrave (1611), s.v. *là*, for example, are 'Beneath, below; downe, downewards'), we find that it has been co-opted into the deictic system in an interesting way. Wagner & Pinchon (1962: §494) have a rather odd view of *là-bas* as an 'intensive variant' ('variante intensive') of *là*; but in the opinion of many commentators for whom *(i)ci* and *là* are to some extent synonymous proximate deictics, *là-bas* is the exponent of remote deixis (see, for instance, Wartburg & Zumthor (1973: §729) and the *Robert* dictionary (Robert 1978), s.v. *là* (t. 4, p. 9)). For Frei (1929: 149), the opposition between *(i)ci* and *là* was a feature of *français traditionnel*, to which corresponded the opposition in *français avancé* between *là* and *là-bas*. But not only does *là-bas* contrast with *là* as an adverb; it has been pressed into service as a demonstrative suffix, as pointed out by Price (1971: 127):

> In practice, the meaning [of the suffix *-là*] is often clear from the context, but where necessary a distinction can be made, between *celui-là* 'this one' and *celui-là-bas* 'that one', *ce train-là* and *ce train-là-bas*, etc.

and by Harris (1978: 96n11):

> At first sight [...] the distinction of proximity [*-ci* vs. *-là*] may appear to be neutralized. In practice, however, whenever necessary, forms in *-là* are opposed to those in *-là-bas*. Cf *c'est pas çui-là, m'sieu, c'est çui-là-bas*.

However, if we accept that there is still an opposition between *(i)ci* and *là* in French (as is demonstrated by the examples in §2), that *là-bas* contrasts with *là*, and that *là-bas* does not mean the same as *(i)ci* (which, for present purposes, may be taken as axiomatic), then in both the deictic adverbs and the demonstrative suffixes, we have a three-way contrast between *(i)ci*, *là*, and *là-bas*, which has to be accounted for.

4. Deixis and person

We may be able to tie all these facts together if we examine the possibility of there being a correlation between deixis and person in French. There are, of course, many languages and dialects in which a systematic correlation of this type exists — the following are a sample:

English dialects:	1	this	(see Catford 1965: 37 (N.E. Scots) Barnes (1886: 19) (Dorset))
	2	that	
	3	yon	
Latin:	1	hic	(see Lewis & Short (1879), s.v. *hic* (p. 852), *iste* (p. 1005), and *ille* (p. 884))
	2	iste	
	3	ille	
Tuscan:	1	questo	(see Rohlfs 1968: §491)
	2	cotesto	
	3	quello	
Southern Italian:	1	chistu (etc.)	(see Rohlfs 1968: §494; forms quoted are from the Calabrian dialect)
	2	chissu (etc.)	
	3	chillu (etc.)	
Portuguese:	1	êste	(see Cunha 1972: 235)
	2	êsse	
	3	aquêle	
Spanish:	1	este	(see Real Academia Española (1970), s.v. *este* (p. 581), *ese* (p. 565), and *aquel* (p. 109))
	2	ese	
	3	aquel	
Serbo-Croat	1	òvāj	(see Meillet & Vaillant (1969: 115))
	2	tâj	
	3	ònāj	

Basque:	1	haur	(see Allières (1979: 61))
	2	hori	
	3	hura	
Turkish:	1	bu	(see Lewis (1967: 71))
	2	şu	
	3	o	
Japanese:	1	kono	(see Clarke & Hamamura (1981: 37-38))
	2	sono	
	3	ano	

It could, of course, be argued that there is a correlation between deixis and person in all languages, but that, in languages which have a deictic system containing only two terms, there will clearly be a certain amount of surface syncretism, and any correlation between the deictic terms and the three grammatical persons will be partly hidden or opaque. This may to some extent be the case in French, and almost certainly is the case in English; the following is an attempt to represent schematically a proposed correlation between deixis and person for these two languages:

(ce...)	(i)ci	1	this/here
	là	2	this/here...that/there
	là (-bas)	3	that/there

It is clear that, in any attempt to correlate a two-term deictic system involving remote and proximate terms with the three grammatical persons, the major problems will arise with the second person. In principle, the first person is unambiguously proximate, and the third person unambiguously remote. The second person is, in this context, ambiguous, being both proximate (as a discourse participant) and remote (as a person other than the speaker). Some evidence of this ambiguity and the consequent problems of correlation is provided by Spalatin (1985) in his discussion of the demonstrative systems of Serbo-Croat and English

— he finds that the first-person form *òvăj* and the third-person form *ònăj* are systematically equivalent to 'this' and 'that', respectively; however, the second-person form *tâj* is ambiguous, and may be equivalent to 'this' or 'that', according to the context. Similarly, Benveniste (1966: 254), in a discussion of the relationship between deixis and discourse, claims that the distinction between *ici* and *là* parallels that between the first-person pronoun *je* and the third-person pronoun *il*, but is unspecific about how the second-person pronoun *tu* correlates with the deictic system. Another aspect of the problem is discussed, in general terms, by Frei (1944: 112), who distinguishes between languages in which the proximate deictic term includes both speaker and addressee and those in which it excludes the addressee and refers only to the speaker. (However, Frei places both French and English in the former category, and assumes that the opposition between *celui-ci* and *celui-là* and the opposition between *this [one]* and *that [one]* are identical. We have seen that this assumption is false, and that the deictic systems of the two languages must be analysed in different ways.)

It seems plausible to argue that, in French, *(i)ci* has retreated and is now confined to the first person, whilst *là* has taken over the second person, either leaving a gap for the third person, or, more likely, coming to cover a deictic space which is too large to be covered by a single term. As a result, there has been pressure to create a third-person term, and *là-bas* has been adopted in this rôle. (If we are dealing with a functional chain, it is surely a drag chain of the type suggested, and not the partial push chain implied by the comment of Grevisse (1986: §969), who claims that *là* may be ousting *ici* because it is itself being replaced by *là-bas*.)

How do the data support this hypothesis, and what are its consequences? We may suggest, tentatively, that all the cases discussed above in which *(i)ci*, but not *là*, may occur correspond to a high degree of first-person involvement. This would be the reason for *Viens ici* being the only acceptable form in the context of urgency, impatience, exasperation, or anger; for *Je suis ici* being tautologous and *Je ne suis pas ici* contradictory (except in the metaphorical sense noted above — it is interesting to note the parallels with the English expression *I'm not myself*, which is contradictory in any literal interpretation, and is consequently acceptable only as a metaphor); and for *ici* being the only deictic which can be used to announce one's identity on the telephone to an unknown and unidentified caller. Similarly, the person who says *Monsieur Chirac n'est pas ici* is making a statement, *inter alia*, about their membership of an organization or their presence in a certain place — they are to some extent identifying with the subject of the sentence. *Là*, on the other hand, appears to correspond to a high degree of second-person involvement (or possibly, if we accept the argument that it is the unmarked term, to a low degree of first-person involvement). Thus, in uttering *Je suis là* or *Je ne suis pas là*, the speaker announces his presence or absence in a way which is relevant to the addressee, and the person who says *Monsieur Chirac n'est pas là* is not identifying with the subject (he is possibly looking at the question from the point of view of the enquirer). One can indeed imagine the theoretically possible, albeit inelegant, utterance *Monsieur Chirac est ici, mais il n'est pas là!*

In this view, the opposition between *Viens ici* and *Viens là* would receive a plausible account — both can occur in most contexts, but the former emphasizes the speaker, whilst the latter emphasizes the addressee. The same underlying principle sheds light on other cases in which, on account of their occurrence in the same context with apparently identical spatial reference, *(i)ci* and *là* have been mistakenly analysed as synonyms. The following exchange, heard in Provence, provides a good example:

— Nous, on n'a pas besoin de partir en vacances: on a tout ce qu'il nous faut ici.
— Où ça?
— Ben, là.

— We don't need to go away on holiday; we've got everything we need *ici*.
— Where?
— Er, *là*.'

In the opening sentence of this short dialogue, the point referred to is viewed from the perspective of the speaker. With the intervention of the addressee, the perspective shifts, and the speaker redefines the point in question in terms of the second person. The point itself has not altered; but its relation to the discourse participants has.

An example of a similar phenomenon is provided by a mother instructing her small son to wash a spot of dirt off his face:

> Là... là... là... (*in exasperation*) ici !

At the word *ici*, the mother pointed to the dirt on the child's face, thereby associating herself more clearly with its position. The context of increasing exasperation here is similar to that described in §2 above.

Finally, we quote the following utterance made by a guide at the Musée d'Art moderne in Villeneuve d'Ascq whilst referring to a collection of paintings by Fernand Léger:

> Ici on a ces tableaux-là.
> '*Ici* we have *ces* pictures *là*.'

At the beginning of the utterance, the guide was looking at the paintings; whilst making the utterance, she turned to face her audience. Once again, the point referred to has changed not in terms of its physical location, but in terms of its relationship to the discourse participants.

The examples upon which this hypothesis is based are rather few in number, and have an essentially heuristic value. Uses of deictics which would help to confirm (or refute) the present arguments depend essentially on contrasts, and are not normally to be gleaned from published or recorded material; in these circumstances, one has to rely heavily on the intuitions of native speakers. However, a glimmer of support for the analysis presented in this paper can be found in the comments of a handful of grammarians and lexicographers, although nowhere is the correlation between deixis and person worked out explicitly. Thus, Le Bidois & Le Bidois (1967: §1732) claim that the statement *Je suis là* does not create the same distance between the speaker and the addressee as *Je suis ici;* it conveys the idea that the speaker is within reach of the addressee or at his disposal. The *Logos* dictionary (Girodet 1976), s.v. *là* (t. 2, p. 1757), in one of its glosses of *là*, suggests that it can be used in place of *ici* to signify 'à l'endroit où on est'. A literal translation of this definition is 'in the place where one is' — however, the pronoun *on* has broader reference than English *one*, and, in many registers of French, may encompass the first-person plural (see, for instance, Harris (1978: 122)). The definition might therefore be interpreted as meaning that *là* refers to the place where 'one is' or where 'we are' (including, or at least not excluding, the second person), whilst *ici*, by implication, will refer to the place where 'I am' (excluding the second person). Finally, the *Grand Larousse de la langue française*, s.v. *là* (t. 4, p. 2916), in its second definition of *là*, suggests that the term implies neither remoteness nor a relationship to the first person — without, however, drawing the inference that *ici* does imply a relationship with the first person.

5. Conclusion

Much of the confusion surrounding the meaning and distribution of *(i)ci* and *là* and cognate deictic terms in French can be dissipated if the terms are assumed to be related to the three grammatical persons in the way suggested in this paper. Although further work is required in order to ascertain whether or not this relationship is the *sole* determinant of the reference of the items in question, (for instance, it seems to be impossible for *(i)ci* to be an exponent of remote deixis, whatever the degree of first-person commitment; this constraint may or may not reflect a fundamental incompatibility between the first person and remoteness), it is clear that pragmatic criteria are of importance in determining deictic reference in French.

In a work which appeared after this paper was submitted for publication, Perret (1988: 266-267) claims that *là* is a **situational** adverb in modern French, whilst *ici* and *là-bas* are spatial adverbs, the former **self-referential** ('*sui-référentiel*'), the latter non-self-referential (*'non-sui-référentiel'*). Perret's analysis raises some important points, but cannot account for all the problems addressed in the present paper. I hope to deal with her arguments in a subsequent article.

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CONFIRMATION AND REPAIR: AN INTERACTIONAL ANALYSIS OF REDOING SEQUENCES IN CHILD-ADULT TALK[*]

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Abstract

Much research interest has focussed on the use of expansions in adults' speech to children, and on the ways in which their use may facilitate a child's grammatical development. A detailed investigation of such phenomena in their interactional contexts reveals a wider range of functions to be identifiable than is suggested in the literature. This paper takes a conversation analytic approach to an investigation of 'redoing' sequences (adults' expansions and repeats) in conversations between an adult and a child of 1;6, and explores two ways in which redoing sequences are involved in the initiation of repair.

It is seen that redoings may serve to acknowledge a child utterance and confirm its appropriacy. At the same time, they may initiate phonetic repair on that utterance. Acknowledgements and confirmations from an adult (which may take the form of redoings) are expected by the child. The child is seen to treat an absence of confirmation as an indication of some kind of trouble in her or his prior utterance. In this way the withholding of a confirmatory redoing by an adult prompts the child to effect self-repair.

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1. Introduction

This paper arises as part of a larger investigation, which examines various ways in which the talk which occurs between children and adults makes 'language form' its focus of attention. One of the significant things which adults and children do when they talk together, is that they talk about the language involved in that talk, as they are doing it. Conversations between adults, particularly in certain repair sequences, do address formal aspects of talk; but this happens far more pervasively, and in a somewhat different way, in adult-child talk. And it seems to me that it is this 'language-focussed' quality of child-adult interaction which is crucial to that interaction being the context in and through which language development occurs. Children learn about talking not just through being involved in interactions, but through those interactions themselves dealing with the talk which is their medium, as they take place. A detailed examination of this feature of child-adult interaction, then, is likely to yield important insights into the nature of children's language development.

Redoings are just one, albeit pervasive, phenomenon of adult-child conversations where talk is worked on in this metalinguistic way; and they are the subject of investigation in this paper. Redoings concern what an adult does *after* a child's utterance. The term is a coinage, and requires clarification, particularly with regard to the notions of *expansion* and *repeat*, commonly presented in the literature on child language. The notion of expansion is one which originates with Brown and Bellugi (1964). Expansions are the kind of things which adults typically do when talking with a young child whose speech is seen as 'telegraphic' or abbreviated in some way: they repeat or redo the child's utterance, and also expand on it, filling in, as it were, the missing parts. An example of an expansion, taken from Brown and Bellugi (1964: 141), is the mother's utterance in the following sequence.

(1) Child : Eve lunch
Mother : Eve is having lunch

In the literature, the use of these expansions is often contrasted with the use of non-expanded repeats, where adults repeat the exact words of a child's utterance, without adding anything to it. However, for my data I found this distinction between expansion and repeat to be not a particularly valuable tool for analysing this group of phenomena. It is a distinction based on whether the adult adds something to the child's utterance or not, and, it seems to me, may be telling us more about how telegraphic or how adult-like the child's speech is, than it is about how the interaction between child and adult is organised. In other words, it is a structurally derived distinction which cannot be assumed to have interactional significance. So instead I use the term *redoings* - covering both expansions and repeats, and also some other related phenomena - to designate a class of utterance types in which an adult produces some version of a child's prior utterance, or redoes at least some aspect of it.[1]

Expansions in particular (and repeats to a lesser extent) have attracted a great deal of research interest, because they have been identified as a characteristic feature of a 'motherese' style of speech - a speech register seen as aiding the child's language development. But the weakness which I see in the way these phenomena have been dealt with in the literature, is that they have been assigned communicative functions without any detailed consideration of the interactional contexts in which they occur. For example, expansions have been seen as supplying 'corrective feedback' on a child's incorrect grammar. Whitehurst and Novak (1973: 333), for instance, see these redoings as providing a corrected model for the child, and they note that the child is often observed to imitate the modelled correction. But there is no consideration of whether a redoing is *designed* as a model - whether these sequences are organised by the participants to provide for such an imitation by the child. Another function assigned to redoings is that they check on an adult's understanding of the child's speech - but again, this interpretation has been made, on the whole, without recourse to a detailed investigation of the interactional sequences involved, to establish, for instance, whether these understanding checks are clarification requests, which put the onus for clarification on to the child, and require the child to take a following turn. And redoings have been described as 'interpretative' - as serving to interpret a child's unclear utterances (Ryan 1974: 199). But who are these interpretations for? Are they made for the benefit of the adult (like an understanding check), for the child's own benefit, or (conceivably) are they interpretations serving to clarify the child's speech for some third party witnessing the interaction?[2] These communicative functions, then, have been assigned without due attention to interactional detail.

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[1] I use the phrase 'at least some aspect of it', because there are instances in the data where an adult seems to 'mimic' the child, by redoing certain prosodic features of the child's utterance, like pitch and voice quality - and this can happen where there is no repetition of the child's words. It is not only structural aspects of utterances, then, which can be redone; although it is the redoing of words with which this paper is concerned.

What I am interested to do is to look in more detail than previous researchers have done at the interactional contexts which surround redoings, in order to gather more precise evidence for establishing just how this class of phenomena may be working for the interactional participants involved. What I will show is that these redoing sequences are performing a number of different interactional tasks, and that it doesn't make sense to say simply that they're serving to correct the child's utterance, or interpret the child's utterance, or whatever. 'Confirmation' is another function identified for redoings, and, as I will show, this, too, is a rather simplified account of what is going on. But in this paper I will be focussing on a class of redoings which are in some way working as acknowledgements and confirmations of a child's utterance, receipting and confirming the appropriacy of what the child has said. In particular, I want to examine two different ways in which the use of these confirmatory redoings is bound up with the initiation of repair.

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[2] Wells 1980: 46, suggesting that expansions may have been misleadingly attributed a high frequency of occurrence and an inappropriate significance in earlier studies, reports, 'in our observations expansions only occur with any frequency when a stranger is present - a situation which has occurred in other studies when a researcher has been present during the collection of data'.

2. Data

Before moving on to an analysis of these phenomena, it is worth outlining both the nature of the data being used, and the way they are to be represented.

The observations which gave rise to this paper arose largely from data involving a single child. Initial analysis of other data shows similar patterns to be emerging in other interactional situations, with other children. But most of the illustrations for this paper are taken from data involving the one child - simply because these are the data which I have analysed in most detail so far for these particular phenomena. The child is 1;6, male, and has been audio-recorded interacting with his mother at home, with no observer present, and in as natural a setting as possible, since what I am interested to observe are the everyday conversations which the child is routinely party to.

The transcriptions used to represent this data aim to give a fuller illustration of interactional detail than is usually represented in the literature on child language, by including an indication of such things as inbreaths as prefaces to speech, overlap, and pause length. In general, I follow the conventions for transcript notation developed by Gail Jefferson, widely adopted within Conversation Analysis, and set out in Atkinson and Heritage (1984: ix-xvi). At times, however, a much higher degree of phonetic detail is appropriate - either to illustrate features of the relationship between utterances, or to represent unintelligible speech which cannot be adequately represented in standard orthography. Accordingly, many of the child's utterances are transcribed in some detail, using symbols from the International Phonetic Alphabet, enclosed in square brackets. Turns or features to which attention is being drawn are indicated by an arrow in the left margin. The participants in the cited extracts are as follows:

I : 1;6 (male)
C : 1;11 (female)
J : 3;10 (female) (C and J are sisters)
M : mother (in each case)

3. Analysis

Turning now to an investigation of the data, the remainder of this paper falls into two parts, concerned with two distinct but related conversational phenomena. The first group of things I want to look at is a class of redoing sequences in which the redoing seems to be both confirming the child's utterance, and effecting repair on it.

3.1 Simultaneous Confirmation and Repair

A first observation to make here is that children's utterances are very often followed by acknowledgements from adults. Adults tend to be very attentive to young children's speech, and to respond to their utterances, whenever such a response is appropriate, with an acknowledgement of some kind. It is also worth pointing out that many of a young child's utterances at the one word stage are names, and they are often deictic: they are the kind of utterances which point out objects in the world and name them. Many of the examples of redoings in the data occur during a picture book reading routine, which has something of the character of a naming test. The adult is showing the child pictures, and the child is required to provide an appropriate name. This is an interactional setting, then, in which talk is particularly language-focussed. In this setting it seems that not only acknowledgement, but beyond this some marker of confirmation or disconfirmation, is an appropriate adult response, following the child utterance.

There are a number of forms which these acknowledgements and confirmations may take. They may be non-verbal, and involve head nods and facial expressions, or they may involve minimal vocalisations like *mm hm* or *yeah*. But very often an acknowledgement or confirmation is done with a redoing. And these redoings fall into two broad structural categories - those which occur on their own, and those which are accompanied by a confirmatory marker like yes in the same turn.[3]

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[3] There appear to be significant distributional differences between these two classes of confirmatory redoings - those with *yes* in same turn overwhelmingly

Extracts (2) and (3) below serve as illustrations of the kind of sequences I am concerned with. Both sequences occur when child and adult are looking together at a picture book. Both illustrate an adult redoing serving to confirm the appropriacy of what the child has just said. In (2), the redoing occurs alone, while in (3) the redoing is followed by a *yes* in the same turn.

(2) M: 't 're tho: s: e

(2.0)

I: [ɸ̰̃ˀk·gɵ·ˠʔʈɕ·]

(0.6)

I: [gɒɐ·ɩʔʈ·ɕ · [ʂ··]

→ M: [g:rape (h)

(3) M: righ [t] 's look in your book what's that?

I: [[kʰa·β̰ɐ]]

(6.5)

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occurring at the end of sequences. It seems as if a redoing accompanied by a yes is a designedly sequence-terminating turn, and is a resource which the adult has, for instance, for withholding the business of repair.

I: p:ea: [( )

→ M: [pea: °h °yes°

It seems clear that in both these examples, the redoing is in some way providing confirmation of the child's naming attempt. In both cases, the pitch of the redoing turn falls quite markedly, which is typical of a turn at the end of a sequence rather than, say, a clarification request which projects further talk. In both extracts the redoing follows a *what's that?* type question from the adult, and is in a position where a confirmation of the child's response is appropriate.

A more interesting observation, though, is that for some of these sequences, while redoings are still serving to acknowledge and confirm, they can also, at the same time, be initiating repair. Extract (4) is a case in point. Once again, M is trying to elicit a name from the child in response to a picture.

(4) M: what's that?

(1.0)

I: [kˣɑ˃ː ɕ˖ˡɪ]

→ M: [hɔ̢˖ ᶿʂ̰ː]

→ I: [k˭ʌ̢ʂ̠]

There are four things in particular to notice about this extract:

a) The adult utterance, the redoing [hɔ̢˖ ᶿʂ̰ː], differs significantly from the child's preceding version, [kˣ˖ɑ˃ː ɕ˖ˡɪ]. Most obviously, the adult utterance is a version of *horse*, in contrast to the child's version of ***horsie***, and begins with glottal friction as opposed to velar closure.

b) The child responds to the adult utterance, by supplying a second version, [k˭ʌ̹s̪̆] He need not necessarily have done this.

c) In that response, the child makes a significant modification of his earlier utterance. There are clearly many phonetic differences between the two versions [kˣ'ɑ˒ː ɕ˖ˡɪ] and [k˭ʌ̹s̪̆]

d) Some of those phonetic differences bring the child's second version closer to the adult redoing. Most obviously, the child's second utterance is a version of *horse* rather than *horsie*. Also, the vowel component is raised and advanced, nearer in quality to the adult's [ɔ̹], and the final fricative is markedly similar to that of the adult utterance.

In other words, then, what these observations lead to is that *repair* is going on, and in this case it is phonetic repair. It seems that the adult can confirm an appropriate lexical selection on the part of the child, while repairing the phonetics of the child's utterance. And notice that in extract (4) the child's lexical choice *is* successfully confirmed. While the child produces three very different versions, they are all clearly some version of *horse* (or *horsie*). He makes no attempt to repair his choice of word - just his pronunciation of it.

A similar case is illustrated by the following extract, where the confirmatory function of the redoing is reinforced by an accompanying *yes*.

(5) I: [ʔɵ˙ɹɪç k̬ʰaᵇβ̩əh˙] ((pharyngealised/whispery voice))

M: c: a̱: rrot: ye̱s

The difference here between the child utterance [k̬ʰaᵇβəh˙], and the adult version, *carrot*, is extreme; and if this sequence were to have occurred without the *yes*, I think it would be interpreted as a clear instance of a correction. But alongside the correction the *yes* is a marker of confirmation - a signal to the child that he has said the right thing. So a similar analysis to that presented for extract (4) seems applicable here. *Yes* confirms to the child that he has chosen the right word. But phonetically his utterance needs attention, so the adult uses a redoing. She could, after all, have used *yes* on its own to confirm the child's choice of word, and omitted the redoing altogether. Instead, the use of a redoing gives the adult a chance to repair phonetic aspects of the child's utterance at the same time as confirming its lexical appropriacy. Notice here that the redoing does not project an imitation of the model by the child. The inclusion of *yes* here ends the sequence (see footnote 3), and the business of repair is 'withheld'. It seems, than, that one way in which these redoings work is as a resource which the adult has available for doing some optional (corrective) work on a child's prior utterance.

3.2 The Absence of Acknowledgement/Confirmation Prompts Self-Repair

I turn now to a second way in which the notion of redoings as acknowledgements and confirmations may be bound up with the initiation of repair. In the following cases it is the *non*-occurrence of redoings which is significant.

I first want to claim that children expect acknowledgements and confirmations of their utterances. The following extract illustrates how persistently children may solicit acknowledgements to their utterances when such acknowledgements are not supplied by adult interactants. (The tape begins in the middle of a sequence. Many of J's turns in overlap involve unintelligible singing).

(6) J: ... [(   )
C: ... [loured   g litte↓r

J: ([ )]

C: [coloured glitte] r=

J: ([ )] =

M: [=yea now don't get those out in here though ca:rls plea:se] =

J: = [( )]

C: = [coloured glitter] hh

C: the [coloured gl↑itter]

J: [( )]

(2.0)

C: ° coloured glitter °

(1.0)

J: I would like thi [s o::ne]

C: [coloured [gl] itter

M: [do you want the g↓o:ld pa per=]

C: =( )

M [let's just have a look] 'n' see what e [lse there is,

C: [ ] [(that) the=

C: = coloured gl↑itte↓: r

J: ° >(there we are)<°

C: the coloured glitter

C: the coloured gl [↑itter

M: [yea: t isn't it lo:vely

It seems that M's first response in line six of the extract is somehow not sufficient for C, who persists with different versions of her utterance until it is acknowledged by M with *yea: t isn't it lo: vely.*

Children, then, expect acknowledgements. And other examples show that when a child's utterance is not acknowledged, the child may treat the adult's silence as an indication that there is some kind of problem with the child's utterance. Consider the following extract.

(7) M: what's this.

(0.3)

I: bus:

→ (1.0)

→ I: [tʰːʈʴ̦ɐ ʈʴ̦əʔᵏ̥tɐ [tʴ̦]

M: [tra:ctor yes.

The child's first response, *bus*, to the adult's name-soliciting question does not get receipted. Instead there is a one second pause, after which the child self-repairs from a version of *bus* to a version of *tractor*, which is receipted by M. So the child appears to treat that lack of acknowledgement as an indication from the adult that there is some kind of problem with the child's utterance - and this prompts him to self-repair.

And this seems to be a reasonable assumption on the part of the child, since there is evidence that this adult at least does deal with inappropriate responses on the part of the child by not receipting them, as the following extract shows.

(8) I: ([ )

M: [let's turn over,

I: ([ )

M: [and see what there is on this page

I: n: o: :

M: ye: : ah look ↑oo↓o what's that

(3.0)

I: [ɸəʂ·ʈ̈ ɐɷ   hᵊ>   hɐɕ·ʈ ̈aɷɐ̰: ɷʃɟ]
< >   < ff >

→ (2.0)

I: [tʰɐⁱʈ̹ʰɐ̰   ʈɕaⁱʈ̹ʰɣ̰   [ʈɕaʈ̹ʰa̠]
((whisper))   ((whisper))

M: m   [ye tra:ctor yes 'n' what's that?

The child's unintelligible speech following the adult's *what's that?* question may be a non-contingent comment on some other aspect of his surroundings (its amplitude certainly suggests that it may be an 'aside'): it is impossible to be certain. However, whatever the content of that utterance, I think it is clear that it is *not* an appropriate response to M's question - and she doesn't receipt it. Instead there is a two second pause before the child provides three versions of what is an appropriate response, *tractor*, which the adult does receipt. So it seems as if this child's assumption that a lack of acknowledgement indicates a problem with his prior utterance is a reasonable one, based on the adult's actual behaviour.

A similar phenomenon may be seen in two examples cited earlier. In both cases, a pause without acknowledgement precedes a child's attempt at phonetic self-repair. Consider again extract (2):

(2) M: 't 're tho:_s: e

(2.0)

I: [ɸ̃ˀk·gɵʏʔʈɕ·]

(0.6)

I: [gʋe·ɩʔʈ·ɕ [ʂ'·]

→ M: [g:rape (h)

A 0.6 second silence immediately precedes the child's self-repair from a version of *grape* to a more adult-like version of *grape*. (Certain phonetic features, such as labiodentality around the vocalic component following velar closure, and the final sequence of glottal closure, palatalised alveolar plosion and alveolo-palatal friction, indicate that the child's two utterances are both versions of the same word, *grape*.) The second example appears in the following, which is an extension of extract (4):

(9) M: what's that?

(1.0)

I: [kᵡ'ɑ˖ː ɕ·ˡɪ]

M: [hɔ̰· ᶿʂ̰ː]

I: [k¨ʌ̰ʂ̰]

→ (0.8)

→ I: [h·ɑ˖ɬ̰] °hh ° ( )°

After the child's second attempt, [k¨ʌ̰ʂ̰], there is a 0.8 second silence without acknowledgement from the adult, after which the child produces a third attempt - his most adult-like - at *horse*. And a final example very nicely illustrates the child making this kind of interpretation when an acknowledgement is not forthcoming, even when, for the child, such an interpretation is itself a problem.

(10) I: [ʔə̰ː ɩç˙ dᵊ̰βə̰eɩʂ] bus

(1.5)

I: bi: : k: e [k]

→ (2.0)

→ I: bu:t i̲:s a b̲i̲ke [°hhh

M: [it i̲s a bike ye̲s

Once again the child is naming items from a picture book. The beginning of the child's first turn is unintelligible, but the turn clearly ends with some version of bus. This may be an attempt at a name, or it may be a comment on traffic passing outside. Whichever it is, though, it is not an appropriate name - and M does not receipt it. After a one and a half second pause the child produces an appropriate name, *bike* - either a first try at the naming task or a repaired attempt, depending on the status of his earlier *bus*. But this turn, too, gets no acknowledgement from M. I would claim that it is the two second pause here, without acknowledgement, which prompts the child to make a remonstration that his prior turn was, indeed, appropriate (*bu: t i̲:s a b̲i̲ke*). The adult then acknowledges that *bike* is indeed an appropriate label - although, interestingly, she doesn't address the issue of how the child has interpreted her lack of acknowledgement. In extract (10), then, the child is interpreting the adult's non-acknowledgement as a signal of a problem in his own utterance, and demonstrating that he is making that interpretation, even when perceiving such a signal to be an inappropriate one. In other words, he can remonstrate against the inference he is making from that silence.

4. Concluding Remarks

I hope this study has shown, firstly, the importance of an interactional approach to the study of child language. When the interactional contexts which surround children's utterances are examined in detail, much more evidence becomes available for making claims about how those utterances are functioning for the participants engaged in interaction.

Redoings have been identified as one exemplary feature of the 'language-focussed' nature of child-adult talk. They have been seen to be a resource which the adult has for doing certain kinds of phonetic repair work on children's utterances, at the same time as confirming other aspects of those utterances. And it has been seen that a child expects confirmation to follow appropriate turns in certain sequences, and therefore treats their non-occurrence as locating a trouble source or repairable (Schegloff, Jefferson and Sacks 1977: 363) in her or his own prior speech. Thus, non-acknowledgement may be seen as one form of other-initiation of repair.[4] While the business of acknowledgement and confirmation is not performed exclusively by redoings, it has been seen that very often these are the structures which do this work in adult-child talk.

These redoing sequences, then, perform work on children's utterances. A redoing picks up an utterance and displays it for some kind of work to be done on it. This work may be corrective, evaluative or investigative; may be immediate or delayed; may be undertaken by the 'redoer' or by the speaker of the original utterance. They are one way in which talk gets talked about, attended to, or dealt with in some way *as language*. These redoings, and other features of language-focussed talk, merit further investigation as fundamental features of the kind of talk through and within which language skills develop.

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[4] See Schegloff, Jefferson and Sacks 1977.

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THE RANGE OF GAPPING AND THE STATUS OF AUXILIARIES

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Full verbs and auxiliaries are both subject to GAPPING as in (1) and (2). In the simplest cases this construction type involves apparent ellipsis within one (or more) clausal conjuncts under identity with the finite verb or auxiliary of a preceding conjunct. Gapped conjuncts contain two or more constituents (though the naturalness of examples with more than two constituents is often reduced), and these contrast with corresponding phrases in a preceding conjunct. The contrasting phrases typically carry a tonic or intonational focus.[1] It has often been suggested that the apparent ellipsis must involve at least a verb (cf. Jackendoff 1971, Stillings 1975, Hudson 1976, etc.), most recently by van Oirsouw who uses the term 'verb site' for the medial ellipsis of gapping, which 'always involves deletion of at least a verb' (1987: 123). In line with this general tradition of analysis Pullum and Wilson (1977: 744) followed by Schachter (1983: 148) see in the ellipsis of both full verbs and auxiliaries in this construction straightforward support for the claim that they belong to a wider category 'verb' or [+V]. I will argue that the general tradition of analysis is wrong and that there are in fact straightforward cases of gapping which do not include a verb or auxiliary. The claim that these belong to the same category does not therefore follow directly, though it may follow given further assumptions. But in the first instance the gapping facts support the view that auxiliaries are heads. These facts therefore count against analyses which in-

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[1] See especially Sag (1976), Neijt (1980) and Sag et al. (1985: 156ff.) for examples and discussion of gapping. Van Oirsouw (1987) surveys the literature. In examples I shall sometimes italicize words which contain a tonic or intonational focus. I shall also indicate the apparent site of ellipsis for clarity: this implies no claim about the structure of examples.

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terpret auxiliaries as specifiers, as dependants in VP structure, or otherwise as non-head items.

(1) *John* likes *sausages* and *Paul* _ *beefburgers*.

(2) *John* must eat his *supper* and *Paul* _ finish his *homework*.

Akmajian, Steele and Wasow (1979: 18, note 17) claimed that Pullum and Wilson's argument failed because nouns and adjectives also underwent gapping, so that the construction did not simply represent a generalization across auxiliaries and full verbs. They cited (3) and (4) in support of their claim.[2]

(3) Harry's book about Affix-Hopping and Fred's _ about Psych-Movement will _ revolutionize the field.

(4) Harry became more hostile towards Fred and less _ towards me.

But ellipses within NPs with a genitive, as in (3), cannot be straightforwardly treated as the same phenomenon as gapping. It is an essential property of gapping that it found only in a narrow range of construction types, principally coordinations: it is virtually restricted to occurrence within conjuncts, as appears from (5a-d). Moreover the gap must be 'high' within the conjunct, affecting the highest clause in an example like (1), cf. (6). But ellipses of the type of (3) do not show these restrictions, as is clear from (7). It seems unlikely that they should be generalized with gapping.[3]

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[2] Schachter rebuts Akmajian, Steele and Wasow's claim on the ground that 'verbs can be gapped only when something follows them' whereas the ellipsis of nouns after possessives and adjectives after comparative markers does not require following material (1983: 195f.). But if 'stripping' (as in *John went to the store and (then) Lou*) and gapping are to be accounted for as an essentially unitary phenomenon, as argued in Sag et al. (1985: 156ff.), then this argument does not hold.

[3] Examples like those in (7) seem to me to be widely enough available to give the lie to Jackendoff's (1971) claim that gapping and his 'N-bar gapping' (as in (7)) should be generalized. Such ellipses are, of course, not

(5) a. *John likes bacon. Paul _ eggs.

b. *John likes bacon, although Paul _ eggs.

c. *If John likes bacon, then Paul _ eggs.

d. *If John must eat his supper, then Paul _ finish his homework.

(6) a. *John likes bacon and I know (that) Paul _ eggs.

b. *John must eat his supper, and your mother says (that) Paul _ finish his homework.

(7) a. John's paper on social history was interesting. Paul's _ on the Lollards was not.

b. John's paper on social history was interesting, although Paul's _ on the Lollards was not.

c. John's paper on social history immediately preceded Paul's _ on the Lollards.

d. I enjoyed John's paper on social history, but Mary told me that she had found Paul's _ on the Lollards heavy going.

The distribution of ellipses of type (4) is less clear. It certainly appears outside coordination, though it does not seem generally satisfactory where it is not 'high' in its construction or conjunct, cf. (8). But it does not provide an immediately clear parallel to gapping (though the relationship needs more investigation). And, if it is essentially the same phenomenon as gapping, then it might be accommodated within a

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unrestricted. But the view that extrasyntactic factors have a major role to play here seems plausible, cf. Sag et al. (1985: 164) and references cited there. See note 5 below for discussion of a further argument developed by Neijt (1980).

broadening of Pullum & Wilson's position in which auxiliaries, full verbs and adjectives all belong to [+V] and permit gapping. Thus Akmajian, Steele and Wasow have not carried their point convincingly.

(8) a. Harry became *more* hostile towards *Fred*, though *less* _ towards *me*.

b. (?) Harry became *more* hostile towards *Fred* without seeming any *less* _ towards *me*.

c. (?) He was only a *little* upset about the *first* proposal, and I'm afraid he'll be rather *more* _ about the *second*.

d. ? Being *less* angry with *Mary* just made me *more* _ with her *brother*.

There are, however, two other construction types which show that gapping has a wider range than has been generally assumed. Before we consider these, remember that it is not simply verbs and auxiliaries alone which may gap in the traditional account, but, more generally, a string of elements which includes at least the 'highest' full verb or auxiliary in the conjunct, but which may also include, perhaps in part, complements and modifiers as in (9), subject to a variety of restrictions (for a review of which see especially Sag 1976). I shall say that such examples 'crucially include' the highest verb or auxiliary in question, so that *will* in (9c) is 'crucially included', but *try* is not.

(9) a. John greedily ate the figs, and Mary _ the bananas. (greedily ate)

b. John posted the money on Wednesday, and Paul _ on Thursday. (posted the money)

c. John will try to come on Wednesday, and Paul _ on Thursday. (will try to come)

d. Harry told this story to his mother, and Tom _ to his father. (told this story; from Kuno 1976: 306)

Now consider 'small clause' constructions and exclamative constructions like those in (10) and (11).[4] These apparently show gapping of nouns and adjectives (in (a),(b)) as well as of strings crucially including nouns, adjectives and prepositions (in (c), (d), (e), (f)).[5] Locative adverbs such as *here* and *outside* when they occur as predicates also apparently permit such gapping as do the verbs and auxiliaries of nonfinite complements.

These constructions all seem to have the properties of gapping noted above. They are apparently restricted to coordinate constructions (see (12)), and to cases where the gap is 'high' in the conjunct (see

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[4] I use the descriptively convenient term 'small clause', but do not intend to imply that such sequences should necessarily be analysed as constituents.

[5] There are severe restrictions on the gapping of strings which crucially include N. But these can sometimes be paralleled in the corresponding clauses which crucially include a copula, as below, so that they do not seem to be a special property of the gapping of strings crucially including N.

(a) What! Ford (was) an instigator of attempts to impeach Nixon, and Bush (_) an instigator of attempts to impeach Reagan!

(b) ... and Bush _ of attempts to impeach Reagan!

(c) * ... and Bush _ to impeach Reagan!

(d) * ... and Bush _ Reagan!

This is why I have not followed Neijt (1980: 28ff.) in adducing the similar restrictions on apparent ellipsis in NPs with a genitive (as in my type (3) above) as part of the evidence against identifying these with gapping structures. Neijt points out that the restricted nature of this apparent ellipsis contrasts with the freer gapping of strings which crucially include V. But this is not the relevant comparison. And it is not clear (to me) that there is a relevant distinction when comparison is made with the gapping of strings which crucially include N.

(13)). There seems to be no good reason to reject the straightforward pretheoretical classification of (10) and (11) with instances of gapping.

(10) a. I consider the courts arbiters of law and theologians _ of morals.

b. I thought John happy with his present and Mary _ with hers.

c. I consider Claudius the foul murderer of his brother and Hamlet _ of his uncle.

d. I consider Caesar an instigator of factionalism among slaves, and Spartacus _ among patricians.

e. I thought John happy to be superintended by a man, and Mary _ by a woman.

f. I thought John in a temper with Elizabeth and Paul _ with Mary.

(11) a. What, the courts arbiters of ethics and theologians _ of law!

b. What, John happy with his present and Mary _ with hers!

c. What, Claudius the foul murderer of Polonius and Hamlet _ of his mother!

d. What, Caesar an instigator of factionalism among patricians, and Spartacus _ among slaves!

e. What, John happy to be superintended by a woman, and Mary _ by a man!

f. What, John in a temper with Elizabeth and Paul _ with Mary!

(12) a. *I consider the courts arbiters of law, though theologians _ of morals.

cf. *The courts are arbiters of law, though theologians _ of morals.

b. *I consider Claudius the foul murderer of his brother if Hamlet _ of his uncle.

c. *What, John pleased with Mary because Paul _ with Elizabeth!

(13) a. *The courts are arbiters of law, and I consider theologians _ of morals.

b. *What, John pleased with Mary and you say Paul _ with Elizabeth!

This data seems not previously to have been observed. Indeed discussion seems to have been restricted to gapping in clauses with a verb (which is almost invariably finite), except when authors have considered generalizing the process to 'other' types of ellipsis.[6] But it is clear that the scope of gapping itself is wider than this. It is not restricted to strings which crucially include a verb or auxiliary, and there is therefore no straightforward line of argument from the occurrence of examples like (1) and (2) to a mutual and exclusive supercategory assignment for full verbs and auxiliaries. In fact, it seems unlikely that any essentially categorial restriction on what is crucially included in gapping will be appropriate, whether as a descriptive statement, or in its formalization. It is true that there is an apparent partial restriction on the occurrence of prepositions, in that it does not generally seem possible to gap a preposition while retaining its immediate complement, cf. (14) and contrast (10f), (11f). But, though puzzling, this is perhaps connected to the fact that a gapping remnant may not normally be subcategorized by a preposition in cases like (15). If we leave this aside, it looks rather as if the gapping in (10) and (11), as more generally with finite full verbs, crucially includes something like the highest nonadverbial predicate in the conjunct, so that the status of auxiliaries must be evaluated with reference to whatever is the appropriate generalization here.

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[6] But while this paper was in press, Hudson (1989) appeared, and he does briefly but explicitly consider the gapping of N and A (pp. 86-7). Hudson concludes that what gapping 'centres on' either has a surface subject or is a verb. But as my discussion here demonstrates the phenomenon is wider than this (and open, one would hope, to a more unitary statement, perhaps along the lines sketched below). Note in particular that Hudson's account does not allow for such examples as:

(a) What, always in a temper with Elizabeth and never _ with Mary!
(b) What, one week a supporter of Celtic, and the next week _ of Rangers!

(14) *What, John in the garden and Mary _ the orchard!

(15) a. John relied on Mary, and Paul _ on Martha.

b. *John relied on Mary, and Paul _ Martha.

Can we then go on to say anything about the status of auxiliaries? I think we can argue with some plausibility, given reasonable assumptions, that the ellipsis in gapping crucially includes a head, hence that auxiliaries are heads. Let us consider this first by viewing gapping as a process. Adopting a constituent structure analysis we can see gapping in (1), (9), (10) and (11) above as affecting a sequence NP XP where XP is predicate to NP.[7] For example, in (10a) NP is *theologians*, and XP would be *arbiters of morals*. What is gapped crucially includes a head of the second constituent: its lexical head in (1), (9a) and (10a), a phrasal head in (9b). (I here assume that the adverb phrase in (9b) is generated by *VP --> VP AdvP*.) For some analysts the head of the second constituent may in its turn be the head of S, or of a small clause constituent, or of the exclamative clause, and it may be as the head of the conjunct as a whole that it is crucially included in gapping.[8] But such

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[7] This is not intended as a general characterization, cf. examples such as Sag's *At our house, we play poker, and at Betty's house _ bridge.* (1976: example 3.2.6)

[8] Taking 'be head of' to be a transitive relation. If it is the head of the conjunct as a whole that is involved, then instances of stripping will be straightforwardly included (cf. note 2).

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positions depend on further assumptions which we need not pursue here. Now, since specifiers may not be gapped without their heads, and an auxiliary may be the only item gapped, it looks as if we have some support here for the status of auxiliaries as heads - of their VP, of their S or of both, depending on the analyst's other assumptions.

A second way of looking at this is to consider characterizations of the gapped structure itself. If the gap is to be represented in syntax directly in some way (say, as an empty node or nodes), then it will be characterizable as a head (or as crucially including a head) as just noted. But a minimalist syntactic approach would be simply to generate the major categories which appear in the conjunct as daughters of the conjunct without further structure. This is the analysis followed by Sag et al. (1985). Subcategorizational restrictions, such as that between *relied* and *on Martha* in (15a), follow from their interpretation procedure which involves the substitution of these categories within the structure of a preceding conjunct. Thus this analysis generates for gapped conjuncts an internal structure which lacks a head, and interprets this structure by a process whose effect is to supply a semantic functor (or functors) sufficient to combine the relevant categories. The implication is that what is gapped must crucially include a semantic functor,[9] and, since auxiliaries may be gapped, that they are semantic functors. But being a semantic functor is an important criterion for being a lexical head (see the discussion of Hudson 1987, Warner 1989). So the fact that gapping may crucially include an auxiliary, as in (2) or (9c), strongly suggests that auxiliaries are heads.

Beyond this, there is a further line of argument. If auxiliaries are heads of VP (as argued, among others, by Schachter 1983), and if a head shares the full category of its phrase (as is not necessarily the case if this interrelationship involves 'default inheritance', cf. Gazdar et al. 1985), then auxiliaries are verbs. But that's another and less straightforward story.

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[9] Note that in the system of Gazdar et al. (1985) predicative categories, such as those in (10) and (11), will have the model theoretic type of VP.

I conclude as follows.

(i) Gapping may occur in conjoined 'small clause' constructions and in exclamative clauses, where the ellipsis crucially includes a noun, an adjective, an adverb, or a preposition (provided the preposition does not subcategorize one of the remnants). It is not therefore restricted, as in the traditional account, to ellipses which crucially include a (finite) verb or auxiliary. So it does not of itself provide a direct argument that auxiliaries are verbs.

(ii) Instead, gapping seems to involve an ellipsis which crucially includes a semantic functor, or an item which corresponds to a head of an antecedent conjunct or of its major predicate. Thus the gapping facts support the view that auxiliaries are heads.

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(1) i. J'ai lu [$_{NP}$ beaucoup d' articles] récemment
I've read many (of) articles recently

ii. Pierre s'est brouillé avec [$_{NP}$ trop de collègues]
Pierre has argued with too-many (of) colleagues

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